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DIVERSE DOCTRINES OF EVOLUTION, THEIR RELATION TO THE PRACTICE OF SCIENCE AND OF LIFE[1]

As a fresh unhackneyed subject for an after dinner address I propose to talk on evolution. Some doctrines of evolution are not so hackneyed as others. My own favorite doctrine has been only too much neglected. I now discover with pleased surprise that this very doctrine is coming into fashion. No longer can its exposition be described as a voice or two crying in the wilderness. Philosophical congresses discuss it, eminent zoologists discant upon it; still more significant, it has acquired a name that identifies it. Naturally, therefore, while it is in sight, I seize this opportunity to greet its emergence; to promote its publicity. Therefore, prepare for propaganda. See that your defense complexes are in working order. One needs nowadays to keep them ready for instant use; so you will not complain at my giving them a bit of drill.

The name that the doctrine has acquired is Emergent Evolution. This may be a poor name, but any name is better than being nameless; so one must be thankful. The different ways of conceiving the evolutionary process have diverse bearings upon one's attitude toward the world; upon the temperament and outlook of the student of science; upon the course that science takes. What I wish to do is, not to expound emergent evolution as a doctrine, but to inquire into its bearings on these matters, as compared with those of other ways of looking at evolution; to set forth my own notions of these bearings. You will see that to me this doctrine appears an edifying one. My thesis is that the conscious acceptance of the doctrine of emergent evolution and of its implications would greatly ameliorate biological science as practiced and as preached; would much moderate, mitigate and amend its influence on the human outlook and the practice of living. I speak therefore as a hopeful uplifter.

Evolution is often identified with perfect mechanism; or at least held to be consistent and coincident in its operation with mechanism. According to that doctrine in its perfection, the universe as a whole, or

[1] Address of the retiring chairman of the Zoological Section of the American Association for the Advancement of Science, December 28, 1926.

any limited sample of it, is a set of particles, of one or a few kinds, moving according to certain few invariable laws; the consequent successive groupings of the particles constituting the universe at diverse periods. The process of transformation of the groupings is evolution. From examination of any small sample of the universe, at any time, it is possible to discover the laws of action, of grouping, for all its parts, and for all periods. Consequently, after such an examination of the configuration and motions of the particles at any given moment, the clever observer armed with an adequate computing machine could compute and therefore predict the entire course of evolution; all that will occur or exist at any later period. Evolution is the working of a great machine that never alters its mode of action nor the nature of its product. Science is the examination of what this machine does and produces. Its ideal method is by computation—from a few elementary observations of the constituent particles, their distributions and motions. Science is therefore mainly rationalistic; to but a minimal extent empirical. Nothing essentially new or unexpected can come out of this machine. The thing that hath been is that which shall be, and that which is done is that which shall be done, and there is no new thing under the sun. Such is the soul stirring vision which illumines the path of much of evolutionary science. Even where this vision is not in conscious view, it has induced a subconscious complex which dominates theory and practice. Evolutionary doctrines in large measure adopt the attitudes appropriate to this vision; admit the conclusions based upon it, and regulate their practice accordingly.

The doctrine of emergent evolution rejects this vision as an illusion; explicitly denies the propositions it bodies forth; substitutes for them others that are irreconcilable with them, and with the practical and theoretical conclusions drawn from them. It holds that the conception of the universe as *nothing but* a set of one or a few kinds of particles moving according to a few immutable laws exemplified at any time and anywhere that particles occur, is pitiful in its inadequacy. The notion of computing the entire farther course of evolution from the situation at a given moment, it considers one of those raw and naively incompetent ideas to which at early and unsophisticated periods of culture man is prone. It asserts that the method of science based upon this notion is a false one, not from lack of a sufficiently clever computer with an adequate computing machine; but because at any given time or place the data required for the computations do not exist. It holds that new things, not thus computable, appear as evolution progresses. It holds that with these emerge new methods of action, following new laws; methods not before exemplified; methods that falsify the results of computations based on former methods of action. Concretely, it holds that such new things and new modes of action distinguish the living from the non-living, the sentient from the non-sentient, the reasoning from the non-reasoning, the social from the solitary. It affirms, under correction, that the same is true for the steps from electrons to atoms, from atoms to molecules, from molecules to crystals. It holds that the properties of atoms do indeed depend on those which the electrons have *when they are in the atom;* the properties of molecules on those which the atoms have when *they are in the molecules.* It holds too that the properties of living things depend on those of their physical constituents *when the latter are in living things;* the activities of thinking beings on the action of their physiological constituents when the latter are part of a thinking being; the activities of societies on those of their unit individuals when these individuals form part of the society. But it contends that the constituents of each grade acquire new properties, new modes of action, in becoming part of the "emergent" thing of "higher" grade. It holds that the physics of atoms, of molecules, is not fully known till these are studied in the living as well as in the non-living. It holds that the physiology of bodily constituents separated from the living organism is in essential respects diverse from their physiology *in* the living organism. The constituents of the emergent unit partake of the properties which they showed before becoming parts of that unit; but with additions or modifications. The properties of the emergent unit itself depend on these altered properties of its constituents.

But my present purpose does not require me to expound or defend emergent evolution as a doctrine. That has been done of late by others; by Lloyd Morgan, Ritter, Lovejoy, Wheeler, Parker; by many. I need now but to identify the doctrine. There it stands. What I wish to ask is, in rude parlance, "What *of* it?" What differences does it make? Where does it take us if we adopt it and act upon it,—in place of following the vision of mechanical evolution?

It seems to me to make a great difference as to where we go; a great difference to the practice of science; a great difference to the temperament and bearing of the man of science; a great difference to one's outlook upon life and the universe. Let us look into this.

Look first at the relation of the two kinds of doctrine to our own professional work; to the technique of scientific investigation and formulation. For mechanical evolution, the ideal scientific method is mainly rationalistic. We should require but a few

preliminary observations; of the particles; of their configurations and motions. The rest is a matter of computing, of reasoning. Science ought quickly to leave its toe-touch with observation, with experiment, and soar away; your philosopher is your ideal man of science. Continued recourse to experimentation is a mere device of feeble minds. From a sample of the universe we *ought* to be able to reason out the rest. The experimenters are those of us who can't. It is humiliating. Realizing that we are failures, we perforce continue to experiment, and our inferiority complex continues to grow. The experimenter is a groveling creature, engaged in a practice that is a con-the moment he moves a few inches beyond his experimenter, the more depressed should he be.

This is hard on those of us who find that the only method by which we can progress, in the matters that interest us, is by observation and experiment. I have heard a brilliant experimentalist—a man who has marvellously advanced biological science—say that the moment he moves a few inches beyond his experimental results he goes wrong, as the next experiment shows. He must stick to experiment, or progress stops and error begins. How must such a man beweep his outcast state! How must he wish himself like one more rich in hope and capacity, namely the philosopher! How must he look upon himself and curse his fate, with what he most enjoys contented least! He must, that is, if he accepts the doctrine of mechanical evolution.

Yet in these thoughts almost himself despising, if he but becomes persuaded of the doctrine of emergent evolution—all this changes—his state becomes like to the lark at break of day arising; and now he scorns to change his state with philosophers. The inferiority complex collapses, goes into solution, disappears.

For by the doctrine of emergent evolution, observation and experiment are the primary and the final methods of science, never to be laid aside. They are *the* methods for learning of the universe. On the basis of what they bring forth, reasoning, computation, may indeed act, so long as these stay within the restricted circle that shows nothing new, nothing emergent, as compared with what has already been considered. But always there is the possibility that emergents have come into the circle; always must the conclusions be tested by experiment. Reasoning is good; computation is good; but for *what* are they good? They are good as guides to the next experiment, the next observation. The new things that come in evolution, the new modes of action, can not be discovered by ratiocination; only by observation and experiment. When the reasoned conclusion conflicts with experiment, it is the reasoned conclusion that must give way. Herbert Spencer's tragedy of the deduction killed by an observed fact is as typical and necessary an event in science as is the death of the unfit in the evolution of organisms. The man of science must accept as the final word John Hunter's maxim: Don't think; try! Thinking is an instrument, a very fallible instrument, for helping to decide what to try, but the *last* word must be *try*.

The successful experimenter then may lift up his head; he is practicing the highest activity of man; the essential method of science. And so emergent evolution leads to my own favorite doctrine of the radically experimental nature of science. It is as a doctrine of radical experimentalism that I have before discanted upon it. Radical experimentalism leads to emergent evolutionism, as emergent evolutionism leads to radical experimentalism; they are indeed obverse views of one doctrine. To this joyful meeting and coalescence I must in a few moments allow myself to come back.

But what a difference, too, acceptance of the doctrine of emergent evolution will make in our conception of the relation between the living and the non-living! What a difference it will make in the practice of investigating the properties and activities of organisms. No longer will it be held that the only sound method of learning about the organic is to study the inorganic. No longer will the conviction prevail that the best interpreter of the living is he who confines himself to the study of what is not living. That notion has been the curse of biological science; condemning it to move in pretentious superficialities. Emergent evolution demands a study of the inorganic as well as of the organic; but no more. It demands a study of the inorganic for the sake of the organic. Since the activities of constituent parts in the living are modifications of their simpler actions in the organic and since many of the simpler actions too persist in organisms, a knowledge of the inorganic must continue to be the foundation on which to build an understanding of the organic. But the constituent parts act differently within the organic, for they have come into new relations; the laws of action of atoms and molecules are not fully known till they are examined in organisms. The experimental method is as valid and as indispensable in the living as it is in the non-living. Any result which it yields in the living is as sound and may be as ultimate, as anything it discovers in the non-living. The doctrine of emergent evolution makes the biologist loyal to experimentation and observation in his own field of work, whatever is found in other fields. Courage and defiance sprout in his soul in place of timorous subservience to the inorganic. No longer can the biologist be bullied into suppressing observed results because they are not discovered nor expected from work on the non-living parts of nature. No longer will he feel a sense of

criminality in speaking of relations that are obvious in the living, for the reason that they are not seen in the non-living. Biology becomes a science in its own right—not through rejection of the experimental method but through undeviating allegiance to it. The doctrine of emergent evolution is the Declaration of Independence for biological science.

And *within* the realm of biological science a parallel result must follow. Different organisms, different societies of organisms, are diverse emergents, showing diverse systems of relations and consequent diverse methods of action. No longer must it be held that what is true for one organism is necessarily true for another. No longer will the investigator expect by a single crucial experiment to settle a question for the whole organic world. Knowledge of the biology of the oyster is practically not a solid basis for judgment of that of the social insects; this practical fact will be recognized as theoretically significant; as a fact typical of biological science; not something to be minimized and explained away. Organic evolution will be seen as emergent evolution in its most conspicuous and manifold display. In that day resplendent with promise when this is recognized, the practice of facile generalization which honeycombs with error biological science will lose its seductive charm. To generalize will be recognized as the most laborious task in biology, instead of the lightest and simplest. To discover what organisms have in common becomes an object for wide-extended comparative investigation; not a matter for assumptions. Divergent results of experimentation in different organisms are not to be rejected on *a priori* grounds; diversities are as significant as uniformities. This state of affairs, on the one hand a corollary of emergent evolution, is revealed on the other hand by the advance of experimental biology; things living behave themselves as if emergent evolution were a true doctrine.

And all this applies to man as to other organisms; to his conduct, to his social organization, to his prospects and possibilities. Man, like other organisms, is an emergent. His actions may follow the same principles as those of other organisms, or he may act on different principles. This is not a matter for assumptions; it is all a question of fact, to be discovered by experiment; by experience and in no other way. What man has in common with other organisms is to be discovered by investigation and careful comparison, not taken for granted. Wherein he differs from other organisms; wherein he is unique—this stands on the same footing; it is purely a question of observable fact. Data drawn from the study of man are as ultimately valid (provided drawn by equally sound methods) as are those from Amoebae, the frog or the rat; and this whether they are like or diverse from what is found in other organisms. If a student of humanity asserts that man shows certain characteristics, his assertion is not negatived by the fact that no such characteristics are to be found in other organisms. There is no *a priori* ground for sneering at the notion that man in some respects acts on principles diverse from other animals. *A priori* principles of this sort don't go, if emergent evolution is a correct doctrine; such questions are purely matters for investigation. It is true that the investigation of man is difficult; but unhappily that does not make it the less necessary.

Emergent evolution thus relieves the general biologist of that intolerable burden of responsibility that has been forced upon him, the responsibility of speaking oracularly on the problems of human life, on the constitution and conduct of human society; relieves him of the duty of explaining to man what is wrong with him and what he must do to correct the evil situations that he gets into. The biologist who speaks authoritatively on such matters must be a specialist in the biology of the Hominidae, just as the authority on hookworms and liver flukes must be a specialist in helminthology. That is, he must be a student and experimenter, an actor, in the social life of man; he must be an economist, a politician, a historian, as well as a physiologist. For such an authority, if emergent evolution is a correct doctrine, the proper study of mankind is man—man taken of course in his setting as an organism and as part of the world; but yet an extensive and intensive study of man as a distinct emergent; a creature in his own right. Such a biologist will identify the biology, the behavior of man with that of other organisms only critically, after detailed comparative study and demonstration of the respects in which they are identical; and he will give full weight to any evidence that they are diverse. If it is indeed in social organization that we find emergent evolution most manifestly at work; if it *is* here that that which is new in principle most frequently and conspicuously appears, then we shall be cautious in accepting the advice of even the king of the termites on our own social problems; we shall use discretion and take his advice at most as suggestions toward experimentation. For any organism or society separated from others by steps in emergent evolution, the only possible method for progress is by trial and error. In such progress by trial and error will indeed be found free play for the utmost sharpness of vision as to what it is best to try, and for all possible astuteness of judgment as to what has turned out error; but in the end *a trial* it must be, with no antecedent certainty as to results.

And if we are not to set arbitrary bounds for

emergence, then we may be led to admit with Ritter[2] that a particular human individual may be an emergent; a thing set off from all others, in some respects unique; a creature that is a law unto itself; not to be compressed into any general formula. From the acceptance of this, large consequences will be found to flow.

All this will make a revolutionary change in the outlook on certain troublesome human problems that touch directly the man of science; will help him to reconcile being a man of science with being human. A conflict seems to rage between the principles of scientific method and the principles by which human beings act. Science—mechanical science—asserts that all action is determined by the conditions preceding it, in such a way that all action could be predicted from a knowledge of those conditions. There is then no chance of *our* altering what was predictable before we came into existence; all our efforts are quite in vain; we are helpless. Mechanical science thus leads to fatalism; to the extinction of all stimulus to effort, of all man's attempts to guide the course of events. Worse, mechanical science asserts, not only that all action is determined before it occurs, but that it is determined by the physical conditions, by the material situation; that is, it teaches materialism, with all its gross consequences. Whatever happens is determined by the motions of the constituent particles with their immutable laws; it is only on these that computations can be based. Ideas, ideals, purposes, beliefs—all that is mental—are left without function in the scheme of things. They ought not to exist. And if they do, they might as well not. Their existence is unaccountable. Some have therefore adopted toward them Christian Science attitudes, have succeeded in prevailing on themselves to deny that they do exist. This appears like an instance of the strange power of an idea, but for mechanism it is not.

It can not be denied that men speaking in the name of biological science have proclaimed the basic doctrines from which those conclusions have been drawn; from which they are perhaps justly drawn. The immutability of the laws of nature, the theoretical predictability of the future from the past, the denial that anything essentially new can occur—these are almost commonplaces of the schools. The explicability of all that occurs in the living through a knowledge of the laws of the non-living is a dogma in wide circles of biology. The incompetence of the mental to affect physical happenings has become a widely held doctrine, urged by biologists, philosophers and psychologists. The objective examination of behavior, we are told, leaves no role for the psychic; as one physiologist expressed it "the sensations, memory, thoughts,—disappeared like fluttering forms of vapor; —nowhere remained the smallest spot for the psyche." Neal felt that he was opposing generally accepted scientific doctrine when he had the hardihood to maintain that consciousness makes a difference to what happens. So nearly a commonplace has become the doctrine of the inefficacy of the mental that one finds a writer in a philosophical magazine raising timorously and apologetically, with the fear of the biologist in his soul, the questions whether it *may not* after all be true that "purpose makes a difference," and that "intelligence is practical and a source of power." So low has the perfect doctrine of mechanism brought us!

Holding farther than the future could be computed from the past, that the laws of nature are immutable, mechanical science necessarily turns one's eyes exclusively back into the past for judging of the future. Nothing will be exemplified in the future that has not in principle appeared in the past. For man's affairs as for all others, the thing that hath been is the thing that shall be, and that which is done is that which shall be done, and there is no new thing under the sun. The idea of altering humanity, of replacing what has been bad by what will be better, is a romantic fancy, one of those things that have no excuse for existence; an iridescent dream perhaps, but a silly one.

Mingle this perfect doctrine of mechanism, as has been done, with equal parts of the perfect doctrine of natural selection, and you get a potion, a cocktail, with a kick that is warranted to knock out ethics and civilization. Warfare and destruction have been the means of advance; the laws of nature are immutable; this then must continue. Rapine and murder are the means of progress. To try to stop them is to try to change the laws of nature; is to try to stop evolution. Gentleness, pity, humility and the rest of the "slave virtues" are mere weaknesses deserving of destruction and certain to get their deserts. The only conduct that is justified is that whose powerful violence leads to triumph in the struggle for existence. Ethics does not exist in the universe of perfect evolutionary mechanism; from the latter we learn the opposite of everything by which we aspire to guide our daily lives and to organize society. Such aspirations are mere sickly longings to oppose the course of nature; quite without effect on the course of events.

The tree that bears all these handsome fruits has its roots in the determinism of events, as conceived by mechanism; in the doctrine that whatever happens is determined by the pre-existing situation and could be computed from it. Only if we accept determinism is science possible, and these things, it is urged, follow from determinism.

[2] "The Unity of the Organism," Vol. 2.

I believe that there is no escape from the conclusion that only on the basis of determinism is science possible; only on the basis of determinism is learning by experience possible. Determinism can not be given up without to the same extent giving up the possibility of science. A general abandonment of the conviction that what happens depends on the conditions,—with action based upon that abandonment—would go farther than any other conceivable step to drive man into savagery; nay, to bring about his immediate extinction; and just so far as indeterminism takes the place of determinism is that goal approached. What humanity needs is not less, but infinitely more, realization of the fact that what occurs depends on the conditions; a large proportion of its ills result from lack of that realization. Only through action based on that realization is man freed from his slavery to the forces of nature; only through that is he led to investigate, to invent; only through that does conduct become responsible. Indeterminism signifies the lack of all basis for learning, for science; it means absolute irresponsibility in conduct. We can not give up determinism in science or in life. If the doctrine of mechanism had a monopoly of determinism, then indeed a shift to any other doctrine would have disastrous consequences. There is an impression that mechanism does have such a monopoly; and this impression goes far in accounting for its hold on the minds of men of science.

But this impression has no justification. Emergent evolution makes no difficulties for the thoroughgoing experimental determinism on which depends the possibility of science. It is as things come into new relations that their properties change. Diversities in emergents follow experimentally upon preceding diversities in their constituent elements; in the interrelations of those elements. There is always an experimental cause for the change that occurs. The doctrine of emergent evolution, as I have tried to show, is a doctrine of radical experimentalism. Experimentation has no point if there is not determinism. So far as determinism fails, advance by trial and error is impossible. It is only because we can depend on the results of trial that we are able to discover what *is* error.

Now, examination will show that the doctrines set forth above, which involve science and life in a conflict, are not the fruit of that determinism which is required for the building up of an experimental science; which is required for the practice of life. The doctrines of the immutability of the laws of nature; of the computability and predictability of all that will happen; the doctrine that there is nothing new under the sun; the doctrine that ideas, purposes and ideals do not affect what happens; the doctrine that we are helpless to influence the course of events—all these follow only from certain assumptions, not experimentally justifiable, that mechanism makes as to determinism. They follow only from the *a priori* assumption that causality can hold only as between phenomena of certain types; only between the phenomena presented by the particles of matter moving by immutable laws. Mechanical science is forced to this assumption because only thus can it save the computability and predictability of events; and these it must save or itself perish in the struggle for doctrinal existence. Causal relations therefore can not hold between the mental and the physical. The physical can not affect the mental, and *a fortiori* the mental can not affect the physical. For if they did, computability and predictability fail. *Quod erat demonstrandum.*

In all this *a priori* argumentation, mechanism parts company with radically experimental determinism; with determinism based on purely inductive evidence; with the determinism that underlies emergent evolution; with the determinism that makes possible inductive science. That determinism is concerned only with experimentally discoverable relations. The only test as to whether one phenomenon affects another is experiment. Applied temporally, for questions of causation, the test is: remove severally each preceding condition, and observe whether this alters the later phenomena. If it does, this is what we mean by saying that one condition affects another; that one determines another. Such experimental determinism is not concerned with likenesses or differences in kind, as between mental and physical, nor with the conceivability or inconceivability of causal relations between them; it is purely a matter of experiment. It discovers empirically that when two cases differ in some respect, there is to be found a preceding difference to which the later difference is experimentally due. But what result shall follow from a given precedent diversity is known in the first instance only by experience, not necessarily by reasoning or computation. Emergent evolution, or radical experimentalism, leaves untouched the probable universality of this relation of experimental determinism. It leaves this universality an open question, the answer to which is to be approached, like the answer to all questions of inductive science, by continued experimentation. So far as it attempts to anticipate this answer, it asserts universal determinism only as an induction—the widest of scientific inductions—based upon cumulative evidence from experiments in all the fields of nature.

If we rely solely upon experiment, the production of mental diversities by preceding diversities in physical conditions is the commonest experience of mankind; a brick dropped on the foot yields other mental

results from a feather so dropped. Such determinism stands up under the most critical analysis, so far as the latter is restricted to experimental considerations. In so far as this relation is a universal one; in so far as diversities in mental state are always preceded and accompanied by diversities of physical state, an analysis of the situation shows that experimental determinism also holds for the production of physical diversities by preceding mental diversities; for experimental determination of the physical by the mental. One result follows when a certain mental state precedes; another when another mental state precedes, and this is exactly experimental determinism. No ground based on experimental analysis can be alleged for the assertion that the mental does not affect the physical; this is a purely *a priori* notion.

According therefore to radical experimentalism, consciousness does make a difference to what happens; particular types of consciousness make a difference. Emergent evolution asserts this from another point of view; the conscious emergent is one that acts on different principles from the unconscious one; the two doctrines are here again one. Emergent evolution so does away with that monstrous absurdity that has so long been a reproach to biological science; the doctrine that ideas, ideals, purposes have no effect on behavior. The mental determines what happens as does any other determiner.

This carries with it a very different outlook on nature and life from that implied by the contrary view. The situation completely changes as to fatalism and materialism. Among the determining factors for the happenings in nature are those that we call mental. Thought, purpose, ideals, conscience, do alter what happens. That is, a man with an idea behaves diversely from a man without one; just as a man grasping the electrodes of a powerful battery behaves differently from one not connected up with the battery. As suspected by the philosopher quoted earlier, purpose does make a difference; intelligence *is* practical and a source of power. It is not strictly true that "popular approval or disapproval will not alter the course of nature"; that is one of the main factors in the course of nature as including man. The desires and aspirations of humanity are determiners in the operation of the universe on the same footing with physical determiners.

Furthermore, since a particular emergent individual may be a unique exemplar, mentally and otherwise, he may act in ways that are unique, in ways that are diverse from those of any other individual under the same outer conditions. Such an individual is free from the tyranny of general law; is free from determinism by conditions outside itself; is free to act in accordance with its own nature alone; and yet in its acts there is no breach of experimental determinism.

Such determinism, it is clear, does not imply that what is to come in the future is predictable from what has occurred in the past. The statement that the laws of nature are immutable must not be construed to mean that new laws shall not be exemplified as new conditions arise. Because things have occurred in a certain way in the past it does not follow that they must thus occur in the future. This has not been the history of evolution in the past; there is no ground to expect it to be so in the future. There is nothing in science or scientific method that makes it unreasonable to hope for the appearance in the future of what has not been seen in the past; that is incompatible with striving to realize ideals that have never yet been realized.

Acceptance of emergent evolution, carrying these things with it, will possibly tend to make us dwell more peacefully with our fellow students of the various series of emergents. The physicist, the chemist, shall have his way with the inorganic; the zoologist with animals; the humanist with man; their results supplement one another but need not coincide, for they are studying diverse emergents. Like the lion and the lamb, in that day they shall all lie down together. Patrick[3] suggests that even the fundamentalist and the evolutionist approach one another in this doctrine; but I fear that never that twain shall meet in peace till one is inside the other. The fundamentalist that subtly assails the foundations of science by attempting to destroy the basis on which it rests; or the cruder variety that assaults it with legal and physical restraint—these are the enemy, with whom there is peace only in defeat or victory.

In sum, acceptance of the doctrine of emergent evolution would, I believe, work out to the benefit of science and of humanity. It combines the advantages of mechanism and of vitalism, dismissing the ineptitudes of each. It offers no obstacles to the continued progress of science nor to its formulation. At the same time it sets no limits as to what science shall discover. It recognizes that science is never finished, that it must continue to develop so long as evolution continues. We need not make the doctrine of emergent evolution a dogma; one must hold doctrines experimentally, as he practices science experimentally. But the world behaves as it would if emergent evolution were a correct doctrine. Science I believe would find itself more adequate to that world if it too would try for a while behaving as if emergent evolution were a correct doctrine.

H. S. JENNINGS

THE JOHNS HOPKINS UNIVERSITY

[3] "The Convergence of Evolution and Fundamentalism," *The Scientific Monthly*, July, 1926.

THE NATION AND SCIENCE[1]

I SHOULD like to discuss with you for a few moments certain relationships of pure and applied science research to public policies and above all the national necessity for enlarged activities in support of pure science research.

Huxley was perhaps not the first but at least he was the most forceful in his demand that preliminary to all understanding and development of thought was a definition of terms. Men in the scientific world will have no difficulty in making a distinction between the fields of pure and applied science. It is, however, not so clear in industry or in our governmental relations and sometimes even in our educational institutions.

At least for the practical purposes of this discussion I think we may make this definition—that pure science research is the search for new fundamental natural law and substance—while applied science is clearly enough the application of these discoveries to practical use. Pure science is the raw material of applied science. And the two callings depart widely in their motivating impulses, their personnel, their character, their support and their economic setting. And these differences are the root of our problem.

As a nation we have not been remiss in our support of applied science. We have contributed our full measure of invention and improvement in the application of physics, in mechanics, in biology and chemistry and we have made contributions to the world in applied economics and sociology.

Business and industry have realized the vivid values of the application of scientific discoveries. To further it in twelve years our individual industries have increased their research laboratories from less than 100 to over 500. They are bringing such values that they are increasing monthly. Our federal and state governments to-day support great laboratories, research departments and experimental stations, all devoted to applications of science to the many problems of industry and agriculture. They are one of the great elements in our gigantic strides in national efficiency. The results are magnificent. The new inventions, labor saving devices, improvements of all sorts in machines and processes in developing agriculture and promoting health are steadily cheapening cost of production; increasing standards of living, stabilizing industrial output, enabling us to hold our own in foreign trade; and lengthening human life and decreasing suffering. But all these laboratories and experiment stations are devoted to the application of science, not to fundamental research. Yet the raw material for these laboratories comes alone from the ranks of our men of pure science whose efforts are supported almost wholly in our universities, colleges and a few scientific institutions.

We are spending in industry, in government, national and local, probably $200,000,000 a year in search for applications of scientific knowledge—with perhaps 30,000 men engaged in the work.

I should like to emphasize this differentiation a little more to my non-scientific audience. Faraday in the pursuit of fundamental law discovered that energy could be transformed into electricity through induction. It remained for Edison, Thomson, Balle, Siemens and many score of others to bring forth the great line of inventions which applied this discovery from dynamo to electric light, the electric railway, the telegraph, telephone and a thousand other uses which have brought such blessings to all humanity. It was Hertz who made the fundamental discovery that electric waves may traverse the ether. It was Marconi and DeForest who transformed this discovery into the radio industry. It was Becquerel who discovered the radioactivity of certain substances and Professor and Madame Curie who discovered and isolated radium. It was Dr. Kelly who applied these discoveries to the healing art and to industrial service. It was Perkins who discovered the colors in coal tar by-products. It was German industrial chemists who made the inventions which developed our modern dye industry. It was Pasteur who discovered that by the use of aniline dyes he could secure differentiation in colors of different cells, and this led to the discovery of bacilli and germs, and it was Koch and Ehrlich who developed from this fundamental discovery the treatment of disease by anti-toxins.

And so I could traverse at great length these examples of the boundaries and the relations of these fields of pure and applied science.

There is a wide difference in the mental approach of the men engaged in these two fields of scientific work. The men in pure science are exploring the frontiers of knowledge and they must necessarily do so without respect to reward or to its so-called practical benefits, whereas the men engaged in applied science research have long since demonstrated that it pays in immediate returns. It brings such direct rewards as to generate its own steam mostly through the Patent Office. There is seldom any direct financial profit in pure science research, although its ultimate results are the maintenance of our modern civilization and are the hopes for the future.

For all the support of pure science research we have depended upon three sources—that the rest of the world would bear this burden of fundamental discovery for us, that universities would carry it as a

[1] Address before the Society of Sigma Xi and the American Association for the Advancement of Science, Philadelphia, Pa., December 28, 1926.

by-product of education, and that our men of great benevolence would occasionally endow a Smithsonian or a Carnegie Institution or a Rockefeller Institute. Yet the whole sum which we have available to support pure science research is less than $10,000,000 a year, with probably less than 4,000 men engaged in it, most of them dividing their time between it and teaching.

Some months ago our leading scientists in reviewing the organizations of pure science of the country were discouraged to find that their activities had been actually diminished during the last decade, whereas if these laboratories are to furnish the increasing vital stream of discovery to our nation, and our normal part to the world, they should have been greatly enlarged. Moreover, they discovered that the pressures of poverty in Europe were taking a worse toll of pure science abroad.

The causes in the United States are not far to seek. They arise from two directions: First, 80 per cent. of the men devoted to pure science research with us are in our scores of universities and colleges. Our universities have doubled in the number of their students. Their pre-war endowments and income have been depreciated by the falling dollar. New resources have been given many of them, but not enough to handle their new burdens of teaching. All of this has led them to more and more curtailment and the suppression of expansion in pure science research in order that they might attend the immediate problem of education. Thus the four or five thousand men in the United States who had demonstrated their ability for research of this character are not applying themselves in this direction so much as they are applying themselves to the education of the youth. Teaching is a noble occupation, but other men can teach and few men have that quality of mind which can successfully explore the unknown in nature. Not only are our universities compelled to curtail the resources they should contribute in men and equipment for this patient groping for the sources of fundamental truth because of our educational pressures, but the sudden growth of industrial laboratories themselves and the larger salaries they offer have in themselves endangered pure science by drafting men from the universities. This is no complaint against our great industries and their fine vision of the application of science. It simply means we must strengthen the first line of industrial advancement—pure science research.

These men of pure science are the most precious assets of our country and their diversion to teaching and applied science reduces the productivity which they could and should give to the nation. It is no fault of their own but it is the fault of the nation that it does not give to them and to the institutions where they labor a sufficient support.

There is no price that the world could not afford to pay these men who have the originality of mind to carry scientific thought in steps or in strides. They wish no price. They need but opportunity to live and to work. No one can estimate the value to the world of an investigator like Faraday or Pasteur or Millikan. The assets of our whole banking community to-day do not total the values which these men have added to the world's wealth.

Some scientific discoveries and inventions have in the past been the result of the genius struggling in poverty. But poverty does not clarify thought, nor furnish laboratory equipment. Discovery was easier when the continent was new. Discovery nowadays must be builded upon a vast background of scientific knowledge, of liberal equipment. It is stifled where there is lack of staff to do the routine and where valuable time must be devoted to tending the baby or peeling potatoes, or teaching your and my boys. The greatest discoveries of to-day and of the future will be the product of organized research free from the calamity of such distraction.

The day of the genius in the garret has passed, if it ever existed. The advance of science to-day is by the process of accretion. Like the growth of a plant, cell by cell, the adding of fact to fact some day brings forth a blossom of discovery, of illuminating hypothesis or of great generalization. He who enunciates the hypothesis, makes the discovery or formulates the generalization, and thus brings forth the fine blossoms of thought is indeed a genius, but his product is the result of the toil of thousands of men before him. A host of men, great equipment, long patient scientific experiment to build up the structure of knowledge, not stone by stone but grain by grain, is now our only sure road of discovery and invention. We do have the genius in science; he is the most precious of all our citizens. We can not invent him; we can, however, give him a chance to serve.

And the more one observes the more clearly does he see that it is in the soil of pure science that are found the origins of all our modern industry and commerce. In fact our civilization and our large populations are wholly builded upon our scientific discoveries. It is the increased productivity of men which have come from these discoveries that has defeated the prophecies of Malthus. He held that increasing population would constantly lower the standard of living amongst men until the pressure of subsistence upon population would limit its number by starvation. But since his day we have seen the paradox of the growth of population far beyond anything of which he ever dreamed, coupled with a constantly increasing standard of living. This result would be impossible but for the men

of fundamental scientific research and discovery. In fact there is for the future but one contestment in the race with the principle of Malthus, and that is in pure science. If we would have our country increase in its standards of living and at the same time accommodate itself to an increasing population at the rate of more than 15 million each decade we must maintain the output of our pure science laboratories.

The wealth of the country has multiplied far faster than the funds we have given for these purposes. And the funds administered in the nation to-day for it are but a triviality compared to the vast resources that a single discovery places in our hands. We spend more on cosmetics than we do upon safeguarding this mainspring of our future progress.

But to return to my major theme—How are we to secure the much wider and more liberal support to pure science research? It appears to me that we must seek it in three directions—first, from the government both national and state; second, from industry, and third, from an enlargement of private benevolence. We have long since accepted the obligation upon the state to provide universal and free education. We have advanced it further than any nation in the world. Yet the obvious function of education is to organize and transmit our stock of knowledge—it is not primarily concerned with the extension of the borders of knowledge except so far as the process is educational. It seems to me that we must accept the fact that the enlargement of our stock is no less an obligation of the state than its transmission. As a nation we must have this enlargement of stock if we would march forward. And the point of application is more liberal appropriations to our National Bureaus for pure science research instead of the confinement as to-day of these undertakings for applied science work. And we must have the more liberal support of pure science research in our state universities and other publicity-supported institutions.

Our second source of support must come from business and industry. You are aware of the appeal in this particular from the National Academy of Sciences of a year ago—that they might be entrusted with a fund largely for the better support of proved men now engaged in such research in our universities and elsewhere. It is no appeal for charity or benevolence. It is an appeal to self interest, to insurance of every business and industry of its own future. That appeal has been met generously by some of our largest industries; it is under consideration by others; it has been refused by one or two largely because they have not grasped the essential differences between the applied science investigations upon which they are themselves engaged and the pure science which must be the foundation of their own future inventions. A nation with an output of fifty billion annually in commodities which could not be produced but for the discoveries of pure science could well afford, it would seem, to put back a hundredth of one per cent. as an assurance of further progress.

Nor is the interest of a particular industry confined to the science research which appears on its face to be directly in the line of that industry. Practically all industry and all business gains by scientific discovery in any direction. The discoveries which led to the invention of the internal-combustion engine and thus to the automobile have benefited every industry and every business in the United States. Business and industry have an interest in the common pool of scientific research irrespective of its particular field. Those fundamental discoveries of the germ basis of disease, with the load of mortality they have lifted from the race, have lowered the rates of insurance and thus contributed directly to business.

From benevolence we have had the generous support of some individuals to our universities and scientific institutions, but this benevolence has come from dishearteningly meager numbers, as witness the discouraging results of recent appeals from the Smithsonian—the father of American science—and failure of appeals from some of our universities. In a nation of such high appreciation of the value of knowledge, and of such superabundance of private wealth, we can surely hope for that wider understanding which is the basis of constructive action.

And there is something beyond monetary returns in all this. The progress of civilization, as all clear-thinking historians recognize, depends in large degree upon "The increase and diffusion of knowledge among men." Our nation must recognize that its future is not merely a question of applying present-day science to the development of our industries, or to reducing the cost of living, or to eradicating disease and multiplying our harvests, or even to increasing the general diffusion of knowledge. We must add to knowledge, both for the intellectual and spiritual satisfaction that comes from widening the range of human understanding and for the direct practical utilization of these fundamental discoveries. If we would command the advance of our material and, to a considerable degree, of our spiritual life, we must maintain this earnest and organized search for truth. I could base this appeal wholly upon moral and spiritual grounds; the unfolding of beauty, the aspiration after knowledge, the ever widening penetration into the unknown, the discovery of truth, and finally, as Huxley says, "the inculcation of veracity of thought."

No greater challenge has been given to the American people since the Great War than that of our scientific men in the demand for greater facilities. It is an

opportunity to again demonstrate in our government, our business and our private citizens the recognition of a responsibility to our people and the nation greater than that involved in the production of goods or trading in the market.

HERBERT HOOVER

ARNOLD EDWARD ORTMANN

ON the afternoon of January 3, 1927, Dr. Arnold E. Ortmann, curator of invertebrate zoology in the Carnegie Museum and professor of zoology in the University of Pittsburgh, died in the West Penn Hospital, Pittsburgh, in the sixty-fourth year of his age.

He was born in Magdeburg, Prussia, April 8, 1863. He studied at the universities of Kiel, Strassburg and Jena, receiving the degree of doctor of philosophy in the latter institution in 1885. During 1883 he served for one year in the German Army Reserve and retired with the rank of lieutenant of infantry. He was a favorite pupil of Dr. Karl Haeckel, of Jena, and was with him as an assistant on the expedition which Haeckel made to Zanzibar. He subsequently served for a time as instructor in the University of Strassburg. He came to the United States in 1894 and served as curator of invertebrate paleontology in Princeton University from that date until 1903. He became a naturalized citizen of the United States while living at Princeton. From 1909 to 1910 while retaining his position in the Carnegie Museum he served as instructor in zoogeography, and from 1910 until 1925 as professor of physical geography, and from 1925 until his death as professor of zoology in the University of Pittsburgh.

While at Princeton in 1899 he was a member of the Princeton Arctic (Peary Relief) Expedition. He was a member of the American Philosophical Society, of the German Zoologische Gesellschaft, of the Leopoldinisch-Carolinische Akademie der Naturforscher, of the American Society of Naturalists and of the Ecological Society of America. He was a fellow of the American Association for the Advancement of Science, and held membership in many other learned societies. He was the author of numerous monographs and papers upon botany and aquatic invertebrates. He contributed to Bronn's "Klassen und Ordnungen des Tierreiches," writing the portion of that great work which relates to the Decapoda. His report upon the Tertiary Invertebrates of the Princeton Expedition to Patagonia was published in 1902. He made many contributions to the literature of zoogeography and conchology in German and American periodicals. He was the author of a number of important monographs and scientific papers which have been published in the Annals and Memoirs of the Carnegie Museum. During the last twenty years of his life he devoted himself with intense interest to the investigation of the molluscan fauna of the Ohio River and its tributaries, extending his investigations to all the rivers of the eastern United States from New England to the Carolinas and partially exploring the rivers of Georgia, Alabamá and Mississippi. His researches have thrown a great deal of light upon the geology and transformations of the rivers of the eastern half of the United States. His latest research, carried on in the summer of 1926, led him to believe that some of the streams, now discharging their waters into the Atlantic Ocean south of the Chesapeake, originally were tributaries of the great tertiary river represented to-day by Chesapeake Bay, which is a submerged river, and which in Tertiary times drained not only a large portion of eastern Pennsylvania, Maryland and Virginia, but also the northern part of North Carolina.

The death of Dr. Ortmann robs America of one of her most competent and richly informed zoogeographers and students of invertebrate zoology. The list of papers which he has published is long, and his place as a writer upon his favorite themes is firmly fixed in the literature of science. As a field investigator he was scrupulously exact and untiring. As a student in the laboratory he was painstakingly industrious and most scrupulous in keeping his records above reproach. He made extensive collections of the freshwater mussel-shells of North America and other mollusca, which are contained in the Carnegie Museum. He also classified and arranged the mollusca and other invertebrates belonging to the great collections which have been amassed from various sources by the museum as the result of purchase, exchange and collection by expeditions in various parts of the world. As a teacher he aroused enthusiasm, and many postgraduate students in the University of Pittsburgh pursued courses in zoology and physical geography under his care, his classes being for the most part instructed in his laboratory in the museum. In recognition of his attainments and of his contributions to science the degree of Sc.D. was conferred upon him by the University of Pittsburgh in 1911.

Dr. Ortmann made his home during the latter years of his life at No. 6310 Monitor Street, Pittsburgh, Pa.

He is survived by his widow, Anna Zaiss, whom he married at Achern in Baden, December 5, 1894; one son, A. E. Ortmann, Jr., who lives with his mother; and two married daughters, Mrs. Hilda Borgman, of Pittsburgh, and Mrs. Bertha Raeber, of Gloucester, N. J. His sister, Mrs. Hildegarde Ernst, of Pittsburgh, his brother, Dr. Konrad Ortmann, of Torgau, Germany, and four grandchildren also survive.

W. J. HOLLAND

SCIENTIFIC EVENTS

THE PAN-PACIFIC SCIENCE CONGRESS

In the *Volcano Letter* Dr. T. A Jaggar writes:

The Third Pan-Pacific Science Congress opened in the great hall of the Imperial University of Tokyo on October 30 and closed its session on November 11. More than 200 delegates from overseas were present and some 400 delegates from Japan. The congress was organized with extraordinary efficiency under the able leadership of Dr. Sakurai, and the foreign delegates were entertained lavishly by the Imperial Government and by distinguished citizens of the empire. Excursions were provided, before the congress opened, to the Ainu villages of Hokkaido (the northern island of Japan), to the Cretaceous and Tertiary deposits of that island, and to Tarumai volcano, which staged a small explosive eruption just at the time of the congressional visit. Other excursions prior to congress were to Nikko and Hakone, and these beautiful mountain districts were in the full glory of autumnal foliage. Numerous short excursions occupied the week-end of November 6 and 7, and after the congress there were long excursions to central Japan, the Inland Sea and the mines, hot springs and volcanoes of Kyushu. All of this travel was furnished to the overseas delegates at the expense of the Imperial Government, and was even extended to Korea for those delegates who were headed toward China.

The countries represented in the congress were Japan, the United States, Australia, Canada, China, France, Great Britain, Hawaii, Hongkong, Holland, the Dutch East Indies, New Zealand, Papua, the Philippines, the Strait Settlements and Federated Malay States, Russia, Sweden, Peru and Chile.

More than 400 papers were presented in English, the official language, and geophysical sciences were represented by a large number of delegates. Oceanography was accented, and the geological sciences had more representatives than any other group. Japan exhibited instruments and methods, bearing on seismology and earthquake-proof construction, greatly in advance of former practice. At Kyoto there are seismographs with magnification of 50,000, electrical transmission of the motion, and vacuum-tube amplification for registration. Remarkable new accelerographs and vertical-component instruments are under test in the University of Sendai and at the Imperial University of Tokyo. Dr. Imamura has new horizontal pendulums, designed for registering great earthquakes by reducing the amplitude instead of magnifying it, and others of extraordinarily long period by means of special adjusting screws and great height to the supporting columns.

The discussions of the congress developed the need for more extensive translation of scientific works written in obscure languages. Resolutions were passed on motion of those interested in volcanology and local seismology, recommending (1) more published notes at short intervals, (2) more cooperation by radio, (3) more interchange of research workers and (4) translation of far-eastern scientific books through such interchange.

It was provided that the congress of 1929 shall be held in Java.

PROGRAM OF THE AMERICAN ENGINEERING COUNCIL

We learn from the *Electrical World* that the plan to centralize in a single cabinet office all the functions of the federal government which affect public works and the public domain will come before the assembly of the American Engineering Council for approval at the council's annual meeting which will be held in Washington on January 13 and 14. The report of a special committee on organization of federal public works was adopted recently by the council's administrative board at a meeting held at Cornell University and now goes to the assembly, or "engineering parliament," composed of members of 30 national and local engineering societies, for final action after public discussion. Should the proposal be approved, a nation-wide movement among engineers will follow to abolish the present form of the Department of the Interior and to establish a Department of Public Works and Domain. A detailed program of reorganization, accomplishing, it is claimed, large savings in money and increased efficiency in government through a regrouping of bureaus, commissions, offices and other services, has been worked out.

A special meeting of the assembly, Dean Dexter S. Kimball, of Cornell University, president of the council, presiding, has been called for the evening of January 13 to hear the report of a nation-wide study of safety and production in industry. The report will be presented by A. W. Berresford, of Detroit, chairman of the investigation committee. Addresses will be made by Leon P. Alford, of New York, a member of the committee; Joshua Eyre Hannum, research engineer of the Eyesight Conservation Council of New York, and Lawrence W. Wallace, of Washington, executive secretary of the American Engineering Council. Many representatives of industry and the federal government, including Secretary of Labor Davis and the directors of the Bureau of Mines and the Geological Survey, are expected to attend.

Charles M. Schwab, president of the American Society of Mechanical Engineers, will be the chief speaker at the annual dinner of the council, to be held on the evening of January 14 at the Mayflower Hotel. Meetings of the council's executive committee and administrative board will be other events of the sessions. Election of new members of the council will be announced.

THE AMERICAN MUSEUM OF NATURAL HISTORY

THIRTY-THREE expeditions were financed by the American Museum of Natural History last year through the generosity of those interested in scientific work, according to an announcement made at the annual meeting of the trustees at the University Club on January 3. The contributions to the museum totaled $552,000, in addition to more than $2,000,000 of additional endowment.

President Henry Fairfield Osborn, who was elected president for the nineteenth successive year, said that more than $6,000,000 of additional endowment was still needed. Twenty-four trustees contributed $91,850 to educational and scientific costs, and twenty trustees contributed $60,705 for special purposes.

Five new exhibition halls will open shortly, two of them probably by Washington's birthday. The opening of the new halls, the completion of the new Washington Heights subway and the construction of the intermuseum promenade across Central Park are expected to break all attendance records, although in 1926 the number of visitors was 2,070,065. Ten years ago the number was only 847,675.

Promotions and changes in the scientific staff were announced as follows: George H. Sherwood was promoted to the post of director; Dr. William Diller Matthew was appointed curator of geology and paleontology; Walter Granger, curator of fossil mammals; Barnum Brown, curator of fossil reptiles; Robert Cushman Murphy, curator of oceanic birds; Dr. Chester A. Reeds, curator of invertebrate paleontology; Clifford Pope, assistant in herpetology, and Wayne M. Faunce was promoted to assistant to the director and assistant secretary.

George F. Baker was reelected first vice-president; J. P. Morgan, second vice-president, and James H. Perkins, treasurer. Percy R. Pyne was reelected secretary. Frederick F. Brewster, Douglas Burden, Cleveland E. Dodge, Childs Frick and Leonard C. Sanford were reelected trustees for five years. The new trustees elected were Kermit Roosevelt, Oliver C. Jennings and James H. Perkins.

ANTI-EVOLUTION LEGISLATION AND THE AMERICAN ASSOCIATION OF UNIVERSITY PROFESSORS

AT the recent meeting of the American Association of University Professors in Philadelphia a movement was started for more effective cooperation of various groups in opposing the spread of anti-evolution legislation in the states.

The following resolution, offered by Professor A. O. Lovejoy, of the Johns Hopkins University, was adopted:

> *Resolved,* That this association take the initiative in bringing about a more effective cooperation between all groups or organizations interested in opposing legislative restriction on freedom of teaching in state-supported institutions and in defending the principle of the separation of church and state in educational matters.

Another resolution on the same subject, presented by Professor R. H. Johnson, of the University of Pittsburgh, was also adopted:

> *Resolved,* That when some similar organization is formed it should investigate the legality of the action of the Texas State Text-book Commission, which has removed all mention of evolution from text-books in state-supported schools.

The action of the association followed the reading of a report from the committee on freedom of teaching in science, of which Professor S. J. Holmes, of the University of California, is chairman, on the spread of anti-evolution legislation.

> Since the last report of the committee another state, Mississippi, has passed a law prohibiting the teaching of the doctrine of evolution in state-supported schools. The statute is similar to the one passed in Tennessee.

A bill was introduced in the legislature of Louisiana but was defeated, and a similar one will be presented in January to the legislature in Arkansas.

DINNER IN HONOR OF SIR J. J. THOMSON

A CONGRATULATORY dinner was given at Cambridge on December 18 to Sir J. J. Thomson, master of Trinity College, on the occasion of his seventieth birthday. From the report in *Nature* we learn that the celebration was arranged by the Cavendish Society, which consists of past and present students of the Cavendish Laboratory, together with members of the staff. Sir Ernest Rutherford occupied the chair, and about 130 other members of the society were present, while numerous messages were sent by old students who are carrying on the Cavendish tradition in many parts of the world. Something of the spirit which he creates in all who have been associated with him is expressed in the following address, which was presented to Sir Joseph at the dinner, bearing the signatures of 230 of his disciples:

> We, the past and present workers in the Cavendish Laboratory, wish to congratulate you on the completion of your seventieth year. We remember with pride your contributions to theoretical and experimental physics, and especially your pioneer work on the structure of the atom. The additions you have made to knowledge are conspicuous even in this age of remarkable achievement, and have profoundly influenced the history of science. Succeeding to the chair of Clerk Maxwell and Lord Rayleigh, you have made the Cavendish Laboratory, dur-

ing your forty-six years' association with it, an unrivalled center of intellectual activity. We, who have had the privilege of working with you, can not adequately measure our debt. We can only express our grateful appreciation of the help, encouragement and friendship given so freely to all your students, and our hope that you may long live to be an inspiration to the Cavendish Laboratory and the whole world of physical science.

In presenting this address, together with two silver caskets to Sir Joseph and Lady Thomson, Sir Ernest Rutherford related some personal reminiscences of the years—"the happiest in my life"—spent with "J. J." in the Cavendish laboratory, of which he is now the director. Professor P. Langevin, representing men of science from other lands, who have worked in the laboratory, paid a tribute to the guest of the evening. Sir Joseph expressed his thanks in an eloquent speech in which, in generous words, he referred to the unselfish support he had received from all members of his staff, and to the ever-expanding field of scientific research which can never be a terminus but is always revealing new avenues to be explored.

SCIENTIFIC NOTES AND NEWS

THE King John Kasimir University at Lwow, Poland, has conferred the degree of doctor of philosophy, *honoris causa,* on Professor R. A. Millikan, of the California Institute of Technology, and on Professor E. B. Wilson, of the department of zoology in Columbia University. The motion was put forward unanimously by the faculty of science on June 23, the occasion being the forthcoming celebration of the one hundred and fiftieth anniversary of the declaration of the independence of the United States on July 4, 1926.

DR. SIMON FLEXNER, director of the Rockefeller Institute for Medical Research, was elected an honorary member of the Microbiological Society of Vienna at its session held on December 14, 1926.

As a result of the expansion of the U. S. Geological Survey's volcanological work, provided for in appropriations for the current fiscal year, a new station has been established at Mineral, California, at the foot of Mt. Lassen. Mr. R. H. Finch, for a number of years Dr. T. A. Jaggar's assistant at the Kileuea Observatory, Hawaii, is in charge of the new observatory. The equipment includes seismographs installed at the station for continuous records, a portable instrument for field observations, and the usual temperature and time-signal apparatus. The work will include detailed observations of the activities of the volcano, tilt and tremor records, perhaps additional thermal spring observations to supplement the work of Day and Allen, and generally, a study of all of the volcanological phenomena of Lassen.

THE Sofie A. Nordoff-Jung Cancer Prize for 1926 for the best contribution in cancer research has been awarded to Dr. Otto Warburg, director of the department of biology of the Kaiser Wilhelm Institute of Berlin. The members of the award commission were Professors Borst, Doederlein, von Romberg and Sauerbruch, all of the University of Munich.

THE Buchan Prize of the Royal Meteorological Society for 1927 has been awarded to C. K. M. Douglas for papers contributed to the *Quarterly Journal* of the society during the years 1922–25.

DR. HENRY H. GREEN, who is a professor of biochemistry in the department of veterinary science at Transvaal University College at Pretoria, South Africa, has been nominated by the South African Biological Society for the Senior Captain Scott Medal. This medal is awarded annually for outstanding work in some branch of biological science.

WARD SHEPARD, of Washington, D. C., has been awarded the Charles Lathrop Pack forestry prize of $500 for 1926 of the Society of American Foresters.

DR. HANS MOLISCH, professor of botany at the University of Vienna, recently celebrated his seventieth birthday.

THE title of emeritus professor has been conferred upon Thomas Turner, until recently professor of metallurgy in the University of Birmingham.

DR. ARTHUR KEITH, of the U. S. Geological Survey, has been elected president of the American Geological Society.

DR. MORTIER F. BARRUS, extension professor of plant pathology at Cornell University, was elected president of the Phytopathological Society at the recent meeting in Philadelphia.

THE Society of American Bacteriologists has elected the following officers for the year 1927: *President,* Robert S. Breed; *vice-president,* Alice C. Evans; *secretary-treasurer,* James M. Sherman; *councilors,* S. Henry Ayers, Robert E. Buchanan, A. Parker Hitchens, Frank M. Huntoon.

DR. THEODORE H. JACK, dean of the graduate school of Emory University, has been elected president of the Southern Association of Colleges.

DR. JOHN FOOTE, professor of diseases of children, Georgetown University School of Medicine, has been appointed by the secretary of state a delegate to the fifth Pan-American Child Health Congress, which opens in Havana, Cuba, February 13.

LLOYD S. TENNY has been appointed chief of the bureau of agricultural economics of the U. S. Depart-

ment of Agriculture. Mr. Tenny has been acting chief of the bureau since last June.

LORD D'ABERNON has accepted the chairmanship of the industrial fatigue research board, to which he has been appointed by the British Medical Research Council. Mr. William Graham has relinquished the chairmanship of the board under the pressure of other public work, but will remain a member.

N. H. DARTON and A. C. Spencer are on leave of absence from the U. S. Geological Survey and are doing private work in Venezuela and Panama, respectively.

DR. ROY G. WIGGANS, of the department of plant breeding in Cornell University, expects to sail on March 5 for Nanking, China, for a nine months' period to aid in a cooperative enterprise between the University of Nanking, Cornell University and the International Educational Board, in reorganizing and conducting plant breeding at Nanking University.

DR. PHINEAS W. WHITING, of the department of biology of the University of Maine, will spend the spring semester at the Bussey Institution in investigations for the sex committee of the National Research Council.

PROFESSOR HARLEY H. BARTLETT, head of the department of botany and director of the botanical gardens of the University of Michigan, will spend the coming year in botanical exploration in eastern Asia. He will visit Japan, and afterwards carry on field work in Sumatra and in Formosa.

DR. EMERSON MEGRAIL, assistant professor of hygiene and bacteriology in Western Reserve University, has been granted seven months' leave of absence to work at the Lister Institute, London, under the direction of Sir John C. G. Ledingham.

PROFESSOR ANDREW C. LAWSON, president of the Geological Society of America, has returned from a trip around the world.

PROFESSOR RICHARD WILLSTAETTER, until recently professor of chemistry at the University of Munich, is planning to come to the United States in the spring to lecture at Harvard University.

DR. JON ALFRED MJOEN, chairman of the Consultative Eugenics Commission of Norway, expects to arrive in New Orleans about February 20 and will give his first lecture at the University of Iowa shortly afterwards. The arrangements for his lectures are in charge of the American Eugenics Society, New Haven.

PROFESSOR SELIG BRODETSKY, of the University of Leeds, England, gave a talk on the "Einstein Theory of Relativity" at Clark University on January 10. He will later give a series of lectures at Harvard University.

PROFESSOR A. T. JOFFE, head of the department of physics at the Polytechnical Institute at Leningrad and president of the Russian Physical Society, will deliver a series of six lectures at the Massachusetts Institute of Technology during January. Professor Joffe's lectures will include the electrical theory of crystals, types of crystal lattice, elastic and plastic properties of crystals, electrical conductivity of crystals, dielectric strength of crystals and applications of strong fields. Professor Victor Henri, professor of physical chemistry at the University of Zurich, will during February deliver a series of lectures on "The Structure of Molecules and their Chemical Activity."

DR. LOUIS A. BAUER, director of the department of terrestrial magnetism of the Carnegie Institution of Washington, will be the physics departmental lecturer at the school of mines and metallurgy of the University of Missouri during the first week in February, when he will give lectures on "The Earth's Magnetism and Electricity and their Applications."

AT a meeting of the Philosophical Society of Washington on January 8, the retiring president, Dr. William Bowie, of the U. S. Coast and Geodetic Survey, gave an address on "The Part played by Isostasy in Geophysics and Geology."

DR. BERGEN DAVIS, professor of physics at Columbia University, gave a lecture before the Franklin Institute, Philadelphia, on January 13, entitled "Refraction of X-Rays." Thomas Midgeley, research chemist, Dayton, Ohio, will address the institute on January 19 on "Anti-Knock Motor Fuel."

DR. IRA N. HOLLIS, formerly head of Worcester Polytechnic Institute and president of The American Society of Mechanical Engineers in 1917, is undertaking a lecture tour through the south. His lecture schedule covers: Birmingham, Ala., January 6; Atlanta, Ga., January 7; Savannah, Ga., January 10, and Jacksonville, Fla., January 11. From Jacksonville Dr. Hollis went to Washington, where he attended the annual dinner of the American Engineering Council on January 13.

DR. A. O. THOMAS, of the department of geology in the University of Iowa, will give a popular lecture under the auspices of the geological museum of the University of Minnesota on January 23, entitled "The Story of the Glacial Period or Ice Age."

ON December 18, Mr. Noel E. Odell, geologist, third Mt. Everest Expedition, delivered an address to the Royal Canadian Institute, on the subject "The Problem of Mount Everest."

DR. CHARLES H. HERTY, adviser to the Chemical Foundation, delivered the second Aldred lecture on "A Chemist's Formula for Industrial Success," on January 9, at the Massachusetts Institute of Technology.

LANSING B. BLOOM, curator of the State Museum of New Mexico, gave an illustrated lecture on "The Indian and the Spaniard in the Southwest" in the Civil Engineering Building of the Johns Hopkins University on January 6.

DR. GEORGE SUMNER HUNTINGTON, until recently professor of anatomy at Columbia University, died on January 5, at the age of sixty-five years.

DR. ARNOLD EDWARD ORTMANN, curator of invertebrate zoology at the Carnegie Museum and professor of physical geography at the University of Pittsburgh, died on January 3, aged sixty-three years.

DR. HENRY G. MAY, head of the department of bacteriology at Rhode Island State College, died on December 23, aged forty years.

DR. EDWIN GARVEY KIRK, head of the department of pathology and surgical pathology at the Chicago Post Graduate Medical School, has died at the age of forty-six years.

SIR FRANCIS FOX, eminent British engineer, died on January 7, at the age of eighty-two years.

FOR the meeting of the British Association which is to be held in Leeds from August 31 to September 7 this year, under the presidency of Sir Arthur Keith, the following sectional presidents have been appointed: Section A (mathematics and physical sciences), Professor E. T. Whittaker; section B (chemistry), Dr. N. V. Sidgwick; section C (geology), Dr. Herbert H. Thomas; section D (zoology), Dr. G. P. Bidder; section E (geography), Dr. R. N. Rudmose Brown; section F (economics), Professor D. H. Macgregor; section G (engineering), Sir J. B. Henderson; section H (anthropology), Professor F. G. Parsons; section I (physiology), Dr. C. G. Douglas; section J (psychology), Dr. W. Brown; section K (botany), Professor F. E. Fritsch; section L (education), The Duchess of Atholl, M.P.; section M (agriculture), Mr. C. G. T. Morison.

THE international committee for arranging the next Conference on Genetics has decided, unanimously, to accept the invitation of the German Society for the Study of Genetics and will hold the next conference in Berlin in 1927, from September 11 to September 18. The conference will be international in character. German, English and French will be the spoken languages. At the first session of the conference permission to use other languages will be considered and decided upon. It is proposed that each of the six morning sessions shall be opened by an address from some leading authority, giving a comprehensive account of the present condition and future outlook of his special branch of research. The choice of lecturer and subject are, at present, still under consideration. The sessions will serve further for communications and papers on important research and experiment. It will be necessary to limit the delivery of these papers to 20 or 30 minutes. Membership in the conference may be obtained by ticket, price 15 RM. The transactions of the conference will be printed and offered to members for 30 RM. For the last day an excursion to Potsdam, closing with a banquet, has been planned. This excursion and banquet are included in the price of the membership ticket. After the conclusion of the conference there will be longer excursions to Halle and to some of the plant-breeding centers. A program and a list of the papers to be presented before the conference will be issued later.

THE faculty of medicine of the University of Paris announces a special international course in public health to be given at the Sorbonne with the cooperation of the health committee of the League of Nations. Lectures will be given in French "on the major present day problems of hygiene and preventive medicine, taking account of the work carried on and the results achieved in various countries," and will include field trips and practical exercises. Among the lecturers are Drs. Bernard, Calmette, Besredka, Levaditi and Ramon, of Paris; Professor Madsen, of Copenhagen; Drs. Nuttall, James and Greenwood, of England; Dr. Stamper, director of health of Jugoslavia; Dr. Saiki, of Japan; Professor Funk, of Warsaw; Dr. Foramitti, of Austria; Drs. Bordet and Glibert, of Belgium; Drs. Rajchman and Carozzi, of Geneva; Professor Selskar Gunn, of the Rockefeller Foundation; Dr. René Sand, of the League of Red Cross Societies, and Professor C.-E. A. Winslow, of Yale University. Professor Winslow sails for Paris on January 29 and will return about March 1.

UNIVERSITY AND EDUCATIONAL NOTES

CHANCELLOR CHARLES ALEXANDER RICHMOND, of Union University, has announced a gift of $100,000 to the Albany Medical College. The gift was made as a memorial to Dr. Cyrus Strong Merrill by his daughter, Mrs. James Monroe Lown, endowing the professorship of pathology in the medical college.

SWARTHMORE COLLEGE has received an additional gift of $100,000 from Morris L. Clothier, of Philadelphia. This is the third gift to the college by members of the Clothier family within the last month, totaling $210,000.

YALE UNIVERSITY, Dartmouth College and Stanford University will each receive $50,000 from the estate left by the late Charles F. Brooker, chairman of the directorate of the American Brass Company.

ACCORDING to the *Journal* of the American Medical Association, a committee of physicians, under the chairmanship of Dr. John Punton, has made a study of the advisability of establishing a medical school as a department of the new Lincoln and Lee University in Kansas City. The committee has recommended that a grade A medical school be established and the cooperation of local physicians in raising the funds will be sought. The alumni of the old University Medical College have volunteered to raise $150,000.

A DIVISION of municipal and industrial research has been established at the Massachusetts Institute of Technology. This division was made possible through a gift of $100,000 made by J. E. Aldred, president of Aldred and Company, of New York City. The work will be in charge of Professor William A. Bassett.

DR. SAMUEL AVERY, chancellor of the University of Nebraska since 1908, has handed in his resignation on account of poor health.

SAMUEL T. DANA, for three years director of the Northeastern Forest Experiment Station with headquarters at the Massachusetts Agricultural College, has been made provisional dean of the new school of forestry at the University of Michigan.

ARTHUR C. MCINTYRE, government research specialist in forestry, has joined the forestry department of the Pennsylvania State College to study research problems in reforestation.

MAURICE B. VISSCHER, assistant professor of physiology at the University of Minnesota, has been appointed associate professor of physiology at the University of Tennessee College of Medicine.

PROFESSOR MARIO BEZZI, the well-known authority on the Diptera of the world, has been appointed professor of zoology and director of the zoological museum in the Royal University of Turin, Italy. He succeeds the late Professor Ermanno Giglio-Tos.

DISCUSSION

EARLY DAYS OF ANTI-VIVISECTION

I. MISS COBBE

"The Nine Circles of the Hell of the Innocent described from the Reports of the Presiding Spirits."[1]

[1] The two outstanding figures in the early history of anti-vivisection are, in England, Miss Cobbe, and in the United States, Mrs. White.

THIS book of 163 pages with the foregoing title was published in London in 1892. The title page also reads "compiled by [Mrs.] G. M. Rhodes with Preface by Frances Power Cobbe."

In it there were described various alleged cruelties classified in nine sections. Hence the sulphurous title, after Dante—and a long way after. Miss Cobbe in the preface (page viii) says, "Nothing has been inserted save verbatim extracts with reference in most cases from the actual reports of the vivisectors themselves, as published in their own books and in the scientific journals, or abridgements of the same."

On reading it, Mr. (later Sir Victor) Horsley found twenty cases including some of his own in which all mention of anesthetics was omitted, in spite of the fact that in every one of these twenty cases it was distinctly stated in the original papers that the animal or animals had been anesthetized.

On October 25, 1892, Mr. Horsley made these facts public in a letter to the London *Times*. In consequence of the exposure of these absolutely false statements the book was withdrawn. A "Second Revised Edition" with an introduction and reply by Dr. Edward Berdoe, F.R.C.S., was issued in the following year.

In the reply Dr. Berdoe takes up each of these twenty cases, quoting first the "objection" and then the "answer." These answers in a number of cases said that the statement of the administration of anesthesia had been "*overlooked*." In other cases he stated that the data were taken "second hand" from other sources than the original papers. He asserted that the compiler [Mrs. Rhodes] had no access to Mr. Horsley's original papers.

To accuse these various scientists of not having administered an anesthetic when the fact that an anesthetic had been used in every case and was clearly stated in the original papers, was a most serious charge of cruelty. Surely such a charge should never be made unless verified. Mrs. Rhodes could easily have consulted the original papers, for she had free access to the libraries of the British Museum and of the several medical libraries at her elbow in London.

Not all the blame should be put on the shoulders of Mrs. Rhodes. As Horsley points out in his letter to the *Times*, Miss Cobbe in two letters signed personally and published in the London *Zoophilist* of November 1, 1890, and March 1, 1892, likewise omits all mention of anesthesia by Dr. Bradford, and by Ballance and Shattuck, though in the original papers the administration of anesthesia was distinctly shown. Moreover, in the second edition, page 143, Miss Cobbe "assumes all responsibility for this book."

In the same issue of the *Times* appears a letter from

Sir Charles S. Sherrington, who has recently retired from the presidency of the Royal Society, the highest scientific distinction in the world. He writes:

I find three instances in which I am by name and deed held up to public abhorrence. From each of the three statements made about me, the employment of anesthesia in my experiment is studiously omitted, although expressly mentioned in each of the published papers. . . . In two out of the three statements I am accredited with inflicting upon living animals and without the employment of anesthetics, a dissection and procedure that I pursued only upon animals already dead.

No further comment is necessary as to such misrepresentation.

II. Mrs. White

In the United States Mrs. Caroline Earle White played as prominent a rôle as did Miss Cobbe in England. She founded, and until her death was the president of the American Anti-Vivisection Society in Philadelphia and was editor of the *Journal of Zoophily,* now called *The Starry Cross.*

Nearly half a century ago Mrs. White called upon me in order to convert me to her views. We discussed vivisection in my office for about two hours. When we parted I said to her as nearly as I can recall:

I want my position to be clearly understood by you. I deem it a professional, a moral, and a Christian duty to thwart your efforts to prevent experimental research, because that is the most valuable and effective weapon in our warfare against disease in man and animals. The Anti-Vivisectionists are, in my opinion, the active enemies of animals as well as of human beings.

Nevertheless, we parted amicably and remained in friendly, though not intimate, personal relations until her death. I have always had a real appreciation of her fine personal character apart from this one blot, which was an obsession.

Some years ago I pointed out that she clearly advocated human vivisection. I quote now from my book on "Animal Experimentation and Medical Progress."[2]

On March 11, 1885, I gave the address at the Commencement of the Woman's Medical College of Pennsylvania. In this address I emphasized the terrible fact that about twenty thousand people were killed annually in India by snakes, especially the cobra. In reply to this address Mrs. White published an "Answer" and on page 10 of this "Answer" is found the following flat-footed advocacy of human vivisection.

"Dr. Keen mentions that in India alone twenty thousand human beings die annually from snake bites and as yet no antidote has been discovered. How can we search intelligently for an antidote, he says, until we know accurately the effects of the poison? I should reply that in order to find out the effects of the poison and to search also for an antidote, the best plan would be for the experimenters to go to India where they could find as large a field for investigation as they require *in the poor victims themselves. Here is an opportunity such as is not often offered for experimenting upon human beings,* since as they would invariably die from the snake bites, *there can be no objection to trying upon them every variety of antidote that can be discovered.* Nothing seems to me less defensible than these experiments on the poison of snake bites upon animals, since it is the one case in which they could be observed *with so much satisfaction and certainty upon man!*

"Such a proposal [I said] is as absurd as it is cruel. Even if the experimenter could afford sufficient time and money to go to India for months . . . how could he arrange to be present when such unexpected accidents occurred [for the cobra has no warning rattle]? How could he have at hand in the jungle, the ether, chemicals, assistants, tables, tents, food and drink, and the necessary yet intricate and delicate instruments? And even if he had all of these, how could he work with the calmness and the orderly deliberation of the laboratory when a fellow human being's life was ebbing away and every minute counted in such a swift poison?"

This is so swift that usually a patient dies in a few minutes. Even were a hospital and all appliances at hand the patient would ordinarily die before careful, painstaking investigation could possibly be made.

Of course this caused an angry protest from the anti-vivisectionists. But no one could more clearly and calmly advocate human vivisection than did Mrs. White in this quotation. Moreover after twenty-seven years of reflection Mrs. White said, "It does not seem to me that this is a cruel suggestion [*i.e.,* experimenting on human beings bitten by snakes] as my only object in it was to benefit the poor natives who die by the thousand every year."[3]

Again further comment is needless.

W. W. Keen

Philadelphia, Pa.

HELIUM IN DEEP DIVING

In view of the reported success obtained by the use of mixtures of helium and oxygen, substituted for air in deep diving and other high pressure work, whereby it is reported by the Bureau of Mines that not only is much greater speed of recovery from exposure to

[2] Houghton Mifflin Company, Boston, Mass., pp. 251–253.

[3] *Boston Medical and Surgical Journal,* July 25, 1912, p. 143.

the helium-oxygen atmosphere attained, with freedom from that painful and dangerous disability, "the bends," than where air is used, but that greater depths of diving are made possible without danger to the divers, it may be interesting to make note of the source of the original suggestion for this use of helium, since no other acknowledgment has been forthcoming.

I may premise by saying that very early in my career while teaching chemistry in the Central High School of Philadelphia, I carried on a series of experimental investigations concerning the inhalation of gases and wrote a paper under the title, "Inhalation of Nitrous Oxide, Nitrogen, Hydrogen and other Gases and Gaseous Mixtures." This was published in the *Medical Times* in Philadelphia, November 15, 1873, or fifty-three years ago. It was almost my first scientific paper and the results detailed in it were pioneer in their nature.

It was natural, therefore, that when in 1919 I found that it was reported or at least claimed that helium was being obtained in large quantities my mind should revert to the early work and especially to the results of breathing mixtures of hydrogen and oxygen, perfect for breathing, but highly dangerous as an explosive mixture. I realized at once that by substituting helium for hydrogen similar advantages as to rapid diffusion might be obtained and that the mixture would be innocuous and non-inflammable.

Interested as I was, I wrote to Dr. W. R. Whitney, of the Research Laboratory, General Electric Company, Schenectady, N. Y., the following letter, which, as will be seen, embodies the idea of using helium and outlines the advantages which would be expected from its use.

August 19, 1919.

Dear Dr. Whitney:

As you probably well know there is a limit to the pressure which men or animals can sustain in a caisson bridge-piers, salvaging sunken vessels, etc. This is usually put at about 65 lbs. to the square inch corresponding to something less than 200 ft. of water. It occurs to me that perhaps an extension of this limit might be obtained by producing an atmosphere containing helium instead of nitrogen, and rearranging as it were the proportion of oxygen with the helium to supply the needs of the animal body for oxygen. The idea is based upon the principle of the superior rapidity of diffusion of the low density gas and if the atmosphere under high pressure contains too much oxygen with the proportion of 1 to 4 as in ordinary air, air itself might possibly be diluted with helium giving somewhat the same result as an artificial atmosphere with helium and oxygen only. Inasmuch as our war development has called attention to the production of helium on a large scale, the idea does not seem to be a very far-fetched one as compared with its status when helium was a very rare gas, not to be obtained in any quantity.

I would like to know what you think of this general idea and whether you can pick a flaw in it. Its actual value would have to be based upon experimental tests on animals.

Very truly yours,
(Signed) ELIHU THOMSON

With all this in mind, I wrote to the Bureau of Mines, in the hope of getting some helium to make the necessary experiments, but did not obtain any.

In the *Chemical News* of December 19, 1919, was a note from Professor J. C. McLennan, of the physics department of the University of Toronto, stating that he was interested in finding uses for "helium" outside of balloon inflation. This seemed to indicate that after all there *was* a surplus of helium. I then wrote to Dr. McLennan as follows:

January 14, 1920.

Dear Sir:

I notice in the issue of *Chemical News*, December 19, 1919, under a note entitled "Helium" it is stated that you have been interested in finding uses for helium outside of balloon inflation. This prompts me to write you saying that I had communicated with our Bureau of Mines in relation to a use for helium based on the idea that after the war there would be a considerable stock of the gas on hand which could be obtained for distribution, if desired. I found that this was not, indeed, the fact; and practically no pure helium (or nearly pure helium) had been produced from the Oklahoma or Texas gas, as I had been given to understand had been done from the various publications. My idea in relation to the matter is on account of its very high diffusive power, as compared with nitrogen, it might be possible to make a mixture of helium and oxygen to be supplied to divers or others working under high pressure, the amount of oxygen being determined by experiments which, when mixed with helium, should allow respiration and much greater freedom in the removal of effete gases from the lungs, the gas interchanged and speeded up from two to three times. Only experiments could determine the value of this suggestion, if it has any; and, of course, it is based on being able to obtain helium fairly free from admixtures and in sufficient quantity to be supplied to a diver. Inasmuch as salvage and caisson work is limited at present to perhaps about 150 feet or a little more, it is thought that by substituting helium for nitrogen in the air breathed this depth might be say increased 50 per cent. or more.

Very truly yours,
(Signed) ELIHU THOMSON

Evidently there was foreshadowed in this letter an important application for helium, an application which if successful must lead to extension of depth in diving and minimizing of the painful results ac-

companying such work. Five years went by when publications of accounts of experiments eminently successful, as carried out by R. R. Sayres, W. P. Yant and J. H. Hildebrand, of the Bureau of Mines, were described.

I then wrote Dr. McLennan, under date of July 31, 1925, as follows:

Dear Professor McLennan:

You will probably recall that on January 14, 1920, I wrote you in response to a request for suggestions as to possible uses for helium, and outlined a possible set of advantages which it might have in working under high pressures, as in caissons or deep sea work. This was about five years ago, or more. I now find that under Serial No. 2670, the Bureau of Mines sends out a report dated February 20, 1925, in which experiments have been made in the line of my suggestion, by R. R. Sayres, W. P. Yant and J. H. Hildebrand. This is somewhat surprising to me in face of the fact that I tried to get from the Bureau of Mines a supply of helium for just such an investigation, but without any success. It surprises me because there is not the least reference to where the idea originally came from, and I would ask you if you know whether the suggestion, as sent to you, was forwarded to the Bureau of Mines in a letter or otherwise.

Awaiting your reply, I am

Very truly yours,
(Signed) ELIHU THOMSON

P. S. As my first scientific paper of fifty years ago or more dealt with the "Inhalation of Gases and Mixtures of Gases," in which field I carried on quite a number of experiments, and which paper was published in the Philadelphia *Medical Times* of March 15, 1873, it is natural that I should have a decided interest in this matter, and that my attention should turn to the possibilities of helium in this connection when such gas was available.

E. T.

and sent the following to the Bureau of Mines:

July 31, 1925.

Dear Sir:

In looking over the report Serial No. 2670 just received, I would like to call attention to the fact that on January 14, 1920, and in answer to a request for suggestions as to the new uses for helium, I wrote Professor McLennan suggesting the helium-air mixture for caisson work. I also, at the same time, made application to the Bureau of Mines for helium and intended to carry out experiments of this kind if a supply was available.

It seems to me that under the circumstances some acknowledgment is due as to the priority in the suggestion of this helium-oxygen mixture, on which I am glad to see experiments have been carried out with such signal success.

Very truly yours,
(Signed) ELIHU THOMSON

Encs.

P. S. As my first scientific paper of fifty years ago or more dealt with the "Inhalation of Gases and Mixtures of Gases," in which field I carried on quite a number of experiments, and which paper was published in the Philadelphia *Medical Times* of March 15, 1873, it is natural that I should have a decided interest in this matter, and that my attention should turn to the possibilities of helium in this connection when such gas was available.

E. T.

To the former I received a gracious acknowledgment and statement that he had called attention to my suggestion in *Nature* some years before.

To the letter addressed to the Bureau of Mines nothing definite as an answer has been received though about a year and a half has passed by. From recent reports, however, we learn that the helium-oxygen mixture has been used for more extended or deeper diving than before and that the sunken submarine, S–51, was successfully brought to the surface by divers supplied with the helium-oxygen atmosphere.

The moral to be drawn from all this is: If you have a good idea, publish it at once, or patent it, or both, in which case it is not so easy for the other fellow coming along years later to adopt it without giving credit where credit is due.

ELIHU THOMSON

SWAMPSCOTT, MASSACHUSETTS

ABOUT THE ACCUSATION OF PLAGIARISM OF THE LATE DIRECTOR OF THE PULKOVO OBSERVATORY, OTTO STRUVE

IN the December number of the *Journal of the Royal Astronomical Society of Canada* there appeared a biographical article by Mr. A. F. Miller, entitled "Camille Flammarion, his Life and his Work," in which the author accuses the late director of the Pulkovo Observatory, Otto Struve, of intentionally assigning to himself another's discovery, the accusation being based on a suspicion once expressed by Flammarion. Messrs. George and Otto Struve published in the March, 1926, number of the same journal, an answer, in which they completely refuted the accusation on the basis of some documents available and fixed the true state of matters.

As Mr. Miller nevertheless persists in holding to his accusation (July-August issue of the *Journal of the Royal Astronomical Society of Canada*), the astronomers of the Pulkovo Observatory feel obliged to protest against such a dishonoring of their late

director, who was highly respected not only as a scientist, but also for his noble spirit.

As some readers may not be acquainted with the controversy, we will briefly recapitulate the state of matters.

Flammarion has mentioned in his "Catalogue des étoiles doubles," issued 1878, that he had discovered independently of O. Struve the irregular motion of the component C in the system of ζ Cancri, but explains it otherwise than Struve, who ascribes it to the influence of a fourth invisible body.

Some years later Flammarion gives in his popular book "Les étoiles et les curiosités du ciel" the following statement: that he had written to Otto Struve asking the latter to communicate his latest observations of ζ Cancri in order to complete the material available. Struve did not answer this letter, but sent some months later to the Paris Academy a paper, in which he assigns to himself the discovery of the irregular motion of the companion. C. Flammarion adds some ironical suggestions concerning singular coincidence of circumstances that he and Otto Struve simultaneously studied ζ Cancri, that they both applied the same method and that Struve made his discovery after receipt of Flammarion's letter.

Mr. Miller raises in his "Biography of Flammarion" this quite unfounded suspicion, and accuses Otto Struve of plagiarism and "dishonorable conduct."

The letter of Flammarion to Struve is still preserved and an authenticated copy of the letter of April 29, 1874, quoted by Flammarion, is at hand *in extenso.*

We can confirm all that has been said by Messrs. Georg and Otto Struve in their reply to Mr. Miller's attack, namely, that the letter as concerns ζ Cancri contains only a request to send new observations of this double star, about which Flammarion writes "c'est celui auquel je tiendrai le plus à cause de son importance comme système triple."

The discovery of an irregular motion is not even mentioned. Besides there was absolutely no need to call Struve's attention to ζ Cancri, as he had observed the star since 1840 and the great leaps in the observations of this component had been indicated in 1855 by Winnecke in the *Astronomische Nachrichten.*

Although Mr. Miller explains in his second note his assertion, Flammarion had communicated to Struve "full particulars" of his discovery, expressed in the December issue of the *Journal of the Royal Astronomical Society of Canada,* by a mistake of his secretary, he seeks to find in the words "il ne répondit pas" a proof of dishonorable behavior by Otto Struve.

We find quite insignificant the fact that Flammarion did not receive an answer to his letter of April 29, 1874, but it may be by an omission on the part of Otto Struve, quite excusable owing to the extent of his correspondence, or by some possible neglect in the post.

Furthermore there is no reason to think that Struve intentionally kept secret his latest observations, as in the same letter Flammarion thanks Struve for the communication of observations of ζ Cancri.

We do not examine here to what extent Flammarion was right in concealing from Otto Struve the results of his own studies of ζ Cancri, while the latter obligingly put at the disposal of Flammarion his unpublished observations of this star.

After the contents of the letter of April 29 became known, there is no more need for further testimony to the fact that Flammarion did not inform Otto Struve of his discovery; thus the suspicion of plagiarism, of which a hint is given by Flammarion himself and which is so categorically expressed by Mr. Miller, has no foundation in fact.

A. A. IVANOFF,
Director of the Pulkovo Observatory

PULCOVO

THE DISSOLUTION OF INSULIN INTO TWO NEW ACTIVE SUBSTANCES

AT the scientific meeting of the State Institute of Hygiene in Warsaw on November 4, 1926, I read a paper on the separation from insulin of two extremely interesting new substances. This preliminary report can be amplified as follows:

From the insulin produced in our institute the clinical unit being 0.07 mg. by an extremely mild fractionation procedure, for the present two substances could be obtained in a crystalline state, which are being designated in the preliminary way as A and B.

The substance A is contained in the insulin in a larger proportion, the yield corresponding to over half of the initial insulin and unlike the usual insulin it decreases the blood sugar in 70 to 80 per cent. of normal rabbits, but having a high initial blood sugar it decreases the blood sugar by 10 to 44 per cent. Similarly in rabbits with low initial blood sugar it produces either no effect or causes increases from 5 to 20 per cent. This observation has led me to apply the substance clinically on diabetics and non-diabetics in conjunction with Dr. Marceli Landsberg, of Warsaw. From the small number of cases investigated it would be premature as yet to reach a definite conclusion, but so far we had increases of blood sugar or no effect in non-diabetics and marked decreases in diabetics.

As to the substance B it represents a new hormone of complicated and not easily understood action.

Substance B when injected or given per os causes marked and lasting increases in blood sugar so that we have produced hyperglycemia and glucosuria in normal rabbits at six doses of 0.2 mg. of substance per day. The substance B causes a high-grade dilution of the blood with enormous retention of water and if one takes that dilution into account blood sugar increases amounted to over 800 per cent. The rabbits eventually died, and we are investigating the pathological changes, especially in the pancreas, on which a report will follow later. As the substance B was found in insulin and the latter hormone in a number of organs and therefore probably in food and as it acts per os one naturally suspects that this substance may have something to do with the causation of at least certain forms of diabetes.

CASIMIR FUNK

STATE SCHOOL OF HYGIENE,
WARSAW, POLAND

QUOTATIONS

"NARCOSAN" AND DRUG ADDICTION

NEWSPAPERS throughout the country holding membership in the North American Newspaper Alliance carried a story, December 15, concerning the discovery by "Dr." A. S. Horovitz of a new remedy for drug addiction known as "narcosan." Since his arrival on these shores in 1913, Horovitz has been continuously identified with attempts to promulgate cures for all sorts of disorders by mixtures of lipoids and vegetable substances of the nature of non-specific proteins. Included in his records are the Horovitz-Beebe "cure" for cancer, the Merrell proteogens for the cure of practically everything and more recently "narcosan," originally brought out about 1920 under the name of "lipoidal substances." Horovitz's present effort to promote "narcosan" as a cure for narcotic addiction is supported by a clinical investigation by Drs. Alexander Lambert, ex-president of the American Medical Association, and Frederick Tilney, one of the editors of the *Archives of Neurology and Psychiatry*. The paper by these investigators appears in the *New York Medical Journal and Record* for the week of December 17. This paper was rejected by the *Journal* of the American Medical Association because the Council of Pharmacy and Chemistry rejected the product known as "lipoidal substances" in 1921, because up to the present time the product has not been resubmitted and is apparently still of unestablished composition, and because the clinical investigations are not set forth in such a manner as to indicate even ordinary controls, such as might have been secured by treating an equal number of patients with the non-specific proteins alone. Furthermore, on their admittance into the hospital, the patients were given a cathartic mixture consisting of seven ingredients, including some of those in the compound vegetable cathartic pill and a few others. Nevertheless, the paper was promptly accepted by the *New York Medical Journal and Record*, and simultaneously with its appearance in that periodical, a complete statement, highly exaggerated, was issued by the North American Newspaper Alliance. This statement appeared in three parts: the first, an account of the Lambert clinical investigations; the second, life stories of some of the patients, and the third, a highly sensational account of the life of A. S. Horovitz, omitting, however, all the points in his record to which reference has been made earlier in this comment. As soon as it was learned in the headquarters office that the newspaper publicity mentioned had been released by the North American Newspaper Alliance, a statement was given to the Associated Press defining the position of the American Medical Association headquarters office in this matter. Perhaps time will reveal sufficient basis in the Horovitz discovery to warrant its acceptance; possibly the clinical investigations made by Drs. Lambert and Tilney have been strictly accurate and scientific; maybe something actually worth while will come from this attempt to control drug addiction. Nevertheless, there is a method which has been repeatedly defined by the American Medical Association as the safe and scientific method of introducing a new proprietary. The American Medical Association has established a council which will act promptly in passing on the claims made for such products and on their worthiness.—*Journal* of the American Medical Association.

SCIENTIFIC BOOKS

Colloid and Capillary Chemistry. By HERBERT FREUNDLICH. Translated from the third German edition by H. Stafford Hatfield. New York, E. P. Dutton and Company, 1926. 886 pages, 156 figures.

THIS monumental work has hitherto been available only in the original German, but its value as a classic has long called for a translation. At first it was styled "Kapillarchemie," and capillary chemistry still receives a great deal of attention from the author. However, colloidally dispersed systems cover more than half the pages of this book.

The physicist will enjoy the author's treatment of the interfaces liquid-gas, liquid-liquid, solid-gas, solid-solid and also the chapters on capillary-electrical phenomena and the properties of interfacial layers. Nor will he be disappointed with the attention given to membrane equilibria and the osmotic pressure of lyophilic sols.

The biologist will find much to attract him and so will the industrialist, as well as the regular colloid chemist.

Naturally the author of the famous Freundlich adsorption formula would present an exhaustive treatment of adsorption, and this is justifiable, for adsorption is the backbone of colloid chemistry. The opposing arguments of Langmuir and Polanyi as to the thickness of adsorbed films, monomolecular or polymolecular, are given fully and fairly.

The author's clear thinking is illustrated by the following statement: "In comparing different adsorbents we must remember that the amount adsorbed, which is referred to unit weight of adsorbent, does not permit of any proper comparison. It includes two quantities which must be separated: first the *actual specific adsorptive power,* that is, the amount adsorbed per square centimeter of surface; and secondly, the *specific surface area,* that is, the extent of the surface of 1 gram of adsorbent."

It is interesting to note (p. 726) that, in using Debye and Scherrer's method of X-ray study of gels, fibers, etc., it is best to arrange ramie in parallel threads.

In discussing membranes and surface films Freundlich insists that semi-permeability can not be a question of a pure sieve action. "With a sieve action one should be able to arrange the membranes in a series in the order of their permeability. But this is by no means the case; a membrane particularly impermeable to the majority of substances may be more permeable to some substances than is a membrane which is otherwise, in general, permeable."

Enzymes receive extensive treatment under the topic, "The Kinetics of Reactions accelerated by Enzymes." Following this is a discussion of the "Inhibition of Biological Processes by Capillary-active Substances."

It is rather surprising to learn (p. 825) that precipitates of the hydroxides of aluminum and ferric iron formed rapidly by addition of ammonia to the corresponding salt solutions are amorphous, while the micellae of Al_2O_3 and Fe_2O_3 sols, formed slowly by hydrolysis, are crystalline (shown by Debye and Scherrer's methods).

On page 837 the author puts the brakes on Loeb's too-enthusiastic, too-general application of Donnan's equilibrium theory.

The thousands of references given in this great treatise add much to its value. But if one is overwhelmed by the 883 pages one can take refuge in Freundlich's little "Elements of Colloidal Chemistry."

HARRY N. HOLMES

OBERLIN COLLEGE

Deep Sea Fishing in New Zealand: Tales of the Angler's ElDorado, New Zealand. By ZANE GREY. New York, Harper Brothers.

MR. ZANE GREY, "the Izaak Walton of the open sea," the leading deep sea angler of the world, has opened a new field, an "Angler's ElDorado," in his experiences in and about the Bay of Islands, on the North Island of New Zealand. This body of water is a fair rival of Santa Catalina, Cape San Lucas and Southern Florida; three great centers of tuna, sailfish and marlins, which Mr. Grey has already explored.

Besides its thrilling interest to anglers, it has much of value to the ichthyologist in its excellent plates and accounts of distribution and habits. All these fishes (some reaching 1,400 pounds) are too large for bottling and only now and then can individuals be properly preserved and mounted. Most studies of them must be made through photographs.

The two species especially treated and figured by Mr. Grey in this work belong both to the genus *Makaira* or "Marlin-spike-fishes." One of these has been very lately named *Makaira zelandica* by Jordan and Evermann, on photographs from the Bay of Islandi, the other, as Mr. Grey asserts, is still unnamed and is called by him "the black marlin" to distinguish it from the striped marlin or *zelandica.* It is closely related to the huge "black marlin" (*Makaira marlina*) of the west coast of Mexico, but its fins are still lower and the spear shorter. In Jordan and Evermann's recent memoir on "The Giant Mackerel-like Fishes" of the world, the New Zealand "black marlin" is provisionally identified with the marlin of South Africa, *Makaira herscheli.* But this species has longer fins and a longer spear.

The generic *Makaira* must be used for the "marlin-spike-fishes," which differ from the sailfishes, *Istiophorus,* in the very low dorsal. *Tetrapturus,* the spearfishes, a third genus, is intermediate, having a low dorsal also, but with the posterior spines relatively elevated, almost as long as those in front. No species of *Tetrapturus* is known from America, but species occur in the Mediterranean, in Hawaii and in Japan.

DAVID STARR JORDAN

SCIENTIFIC APPARATUS AND LABORATORY METHODS

"A F S," A NEW RESIN OF HIGH REFRACTIVE INDEX FOR MOUNTING MICROSCOPIC OBJECTS

A LARGE percentage of objects mounted on glass slides for examination through the microscope depend

for their visibility upon the difference in refractive index of the object and the material in which it is embedded. The greater this difference, the greater is the contrast in the object and the finer is the detail which can be resolved. Also, the higher the refractive index of the embedding material the greater is the depth of focus (penetration) of any objective lens.

If an object has a refractive index of 1.43 (air = 1) it becomes invisible when mounted in material of the same index. It becomes increasingly more contrasty (visible) as the surrounding substance has its index reduced, but the limit in this direction is 1 or a dry mount. Therefore, for this and some other reasons, dry mounts have never been satisfactory. As the refractive index of the surrounding medium is increased above 1.43 the object becomes more and more contrasty. A great deal of research has been conducted in the past to discover a material suitable for mounting purposes and of high index and there is a large literature on the subject. Many chemicals have been investigated and almost the entire series of natural gums, resins and alkaloids. Up to the present time, however, and except for special purposes the microscopist has been limited to two or three natural resins or mixtures of resins, the refractive index of which is very low. He is, therefore, greatly handicapped at the start of his effort, which, in most cases, is to see as much as he can with his microscope. Of the common mountants, Canada balsam has come into almost universal use because it can be procured easily, is chemically stable and is easily manipulated. Long ago it was pointed out that the exudation of the American sweet gum tree, *Liquidambar styraciflua*, was superior to balsam, but it has not come into general use. This resin is sometimes called "styrax," but it should not be confused with an oriental product of that name used in pharmacy. Its refractive index is 1.58; Canada balsam is 1.53.

Even the liquid amber is not sufficiently high for many objects; in fact, it is desirable to have a series of mounting mediums, one end having the maximum attainable refractive index.

After making exhaustive experiments in many directions the assistance of Mr. Paul Ruedrich was solicited and we began a line of search through the synthetic resins. One of particular promise has been discovered and is noted in our records as "A F S." It is composed of analine, formaldehyde and sulphur. A range of refractive index from 1.68 to almost 2.0 has been obtained and after two months' standing it appears to be entirely stable. The discovery was made on October 8, 1926. It is a well-known fact that substances closely related to this resin are the most stable in organic chemistry and have come into wide industrial use within recent years. It can not of course be definitely proved to be stable until after years of observation, but present indications are that it will be. Certainly it will keep unaltered for a period of months. This substance is a yellow resin which can be used in a thick viscous condition or thinned down with aniline or other solvents. It is used in the same manner as Canada balsam and does not offer some of the difficulties encountered with that substance. It may be hardened in the air, in an oven or with stronger heat. Although yellow in color it effectively transmits the apple-green rays for which most microscopic objectives are corrected.

A comparison of the utility of mounting mediums is afforded by the "index of visibility" which is the amount of difference between the refractive index of object mounted and that of the medium used. Thus, if the silica of diatoms be used for illustration, its index of refraction being 1.43, its index of visibility in Canada balsam becomes 1.53 – 1.43 = .10. In liquid amber it is 1.52 – 1.43 = .15. This means, practically, that 50 per cent. greater utility is obtained from a given microscope if the object be mounted in the latter. In this "A F S" synthetic resin the visibility becomes 1.68 – 1.43 = .25, while in a solidified form it becomes 1.88 – 1.43 = .45; all intervening values may be had. Thus a diatom in "A S F" becomes four and a half times or 450 per cent. more visible than it would be in Canada balsam. The same principles apply with all objects, either stained or unstained.

Likewise, increase in depth of focus being directly proportional to the index of refraction of the mounting medium, this factor, so important in photomicrography, becomes almost twice as great in "A F S" as in balsam.

From the optical properties of this material and its ease of manipulation I am forced to the belief that it will enable the human eye to see that which no eye has seen before.

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THE CULTURE MEDIUM FOR DROSOPHILA

Mr. M. Chino, my co-worker on *Drosophila* in our laboratory, has devised a culture medium for the fly which consists of: 100 gr. peeled banana, 100 gr. kôji,[1] 10 gr. agar-agar and 800 cc of water, with a very small quantity of powdered magic yeast added to the mixture. Kôji has to be soaked in water over

[1] Culture of *Aspergillus oryzae* on rice, used for fermenting rice for brewing saké and also for various other purposes in Japan.

night before being prepared for the food. This amount of food is sufficient for about 30 half-pint milk-bottles. We have been using this culture medium for the last few years, and find it quite satisfactory. In fact, it is practically just as good as the banana-agar food for *D. melanogaster* or *D. immigrans,* and is apparently better than the banana for *D. virilis.* The greatest advantage of the food is, however, that it is much cheaper than the banana food, costing only about one third of the latter in Kyoto at least. Thus, in Kyoto:

100 gr. kôji costs ca.	5.5 sen	
100 gr. peeled banana costs ca.	6 sen	(average cost of a year)
10 gr. agar-agar costs ca.	9 sen	
total	20.5 sen	
while, 800 gr. peeled banana costs ca.	48 sen	
16 gr. agar-agar costs ca.	12 sen	
total	60 sen	

We recommend this culture medium to the workers who can easily obtain kôji. We have not yet tried malt as a substitute for kôji, though it might be worth while trying.

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SPECIAL ARTICLES

ON THE ORIGIN OF SUN-SPOT VORTICES

It is well known that sun spots are the visible manifestations of great cyclonic storms in the surface gases of the sun. It is also known that the gases carried around in the sun-spot vortices carry with them great electric charges, apparently of negative sign. In his estimate of the magnitude of the elements of a given sun spot, Carl Störmer[1] concluded that the negative charge over the sun spot area was equivalent to $5.5 \cdot 10^{15}$ electrons over each square centimeter.

The origin of these great cyclones and the source of the charges carried by the revolving gases have never been explained. The only known way by which such a charged area could be isolated upon a good conductor, such as the sun is supposed to be, is by electrostatic induction from a charge upon some other body in the vicinity of the conductor, and aside from this possibility the only conclusion possible in the present state of our knowledge is that the whole surface of the sun is charged to a potential as great, or nearly as great, as the sun-spot area.

There are other reasons for believing the sun to be charged to an enormous negative potential. It is known that the sun has a magnetic field many times as strong as the magnetic field of the earth, and the only known way in which such a field could be maintained is by electrical currents flowing around the sun, presumably caused by negative charges carried around by the sun's rotation.

Another argument for a negatively charged sun is the apparent impossibility of explaining the slight density of gases at the base of the solar corona except by electric repulsion. The corona has every appearance of a gaseous atmosphere several millions of miles in height, yet the gas pressure at its base has been estimated by astronomers as low as 10^{-13} atmosphere, a lower pressure than we are able to produce in our best air pump vacuum.

The explanation most frequently proposed for this low atmospheric pressure is that the coronal gases are supported by radiation pressure, though it has been shown that this assumption is impossible. According to careful measurements, the total radiation of the sun if it were completely absorbed would be capable of supporting a pressure of about 2.3 milligrams to the square centimeter near the sun's surface. The corona was photographed by Maunder[2] in 1898 to a height of at least five million miles above the sun's surface. In order to be supported by radiation pressure the total weight of a column of coronal gas five million miles high and one square centimeter in cross section could weigh only 2.3 milligrams on a body where gravitation is twenty-seven times as great as upon the earth, and provided it absorbed the total solar radiation.

But the solar radiation is only slightly absorbed by the corona. From measurements made on the brightness of the corona it seems to be about one eight-hundred-thousandth that of the sun. This must mean that all the light absorbed and re-emitted and all the light reflected in the corona is about one eight-hundred-thousandth of the total solar radiation.

Schwarzschild,[3] in a theoretical analysis of radiation pressure, showed that the pressure of sunlight upon particles of the dimensions of gas molecules would be insignificant as compared with the sun's gravitation attraction. He accordingly proposed the hypothesis that the solar corona consists of free electrons, and he calculated that such an atmosphere would reflect light of all wave-lengths with equal facility (which is not true of other gases) and that it would partly polarize the reflected light, both of which phenomena appear in the corona. The one objection

[1] *Astrophys. Jour.*, 43, 347 (1916).

[2] Arrhenius, "Kosmische Physik," p. 119.

[3] Sitzungsber, d. K. Bayer, Akad., Math.-Phys. Kl., 31, 293–338 (1901).

which Schwarzschild could find to the electron gas theory of the corona was the great electric charge which it would give to the sun. He estimated that it would require 10^{18} electrons over each square centimeter of the sun's surface.

In 1909, Debye[4] made a more extensive analysis of radiation pressure than had been made by Schwarzschild. He based his analyses upon the classical theory of radiation and upon the Lorentz electron theory, and he reached the same conclusion as did Schwarzschild, *viz.*, that on particles of the dimensions of gas molecules the radiation pressure of sunlight would be insignificant as compared with the gravitational attraction.

Wilhelm Anderson[5] has recently published a series of five papers on the physical nature of the solar corona in which he examines all the coronal theories heretofore proposed and concludes that none except the Schwarzschild theory of an electron gas can be sustained. More recently, Pettit and Nicholson[6] have shown that their measurements of the distribution of energy in the coronal spectrum made during the total eclipse of January 24, 1925, agree within the limits of the probable error of measurement with the calculation of the energy distribution of sunlight as diffused by an atmosphere of electron gas, as computed by Woltjer in Bulletin of the Astronomical Society of The Netherlands, Vol. **3**, p. 103 (1926).

All these researches, as well as those upon the magnetic fields of sun spots and the general magnetic field of the sun, agree in pointing to the conclusion that the surface of the sun must carry an enormous negative electric charge.

In addition to these theoretical considerations, the present writer has an almost continuous record for five years of a diurnal variation in the electrical potential of the earth at Palo Alto which he believes to be due to the electrostatic induction of the sun's negative charge, and a similar, though smaller, lunar diurnal variation which he attributes to the electrostatic induction of the negative charge of the moon.

If the sun is a highly electrified body, the presumption is that the same is true of the planets. In this event there will be a mutual electrostatic induction between the sun and each of the planets. The effect of such planetary induction upon the sun will be a repulsion of the electrified atmosphere of the sun, causing descending currents upon the side toward the planet and ascending currents upon the opposite side. These conditions correspond respectively to the conditions over regions of high and low barometric pressures in the earth's atmosphere. Thus on the side of the sun opposite to the charged planet there will be an area over which there will be ascending currents in the solar atmosphere, and surface winds will be blowing toward this area from all directions. These are exactly the conditions under which cyclonic storms are produced upon the earth, and their tendency will be to produce similar vortex conditions in the solar atmosphere. On the other hand, the descending currents on the side toward the charged planet will aid in suppressing the cyclones already formed.

In *Monthly Notices* of the Royal Astronomical Society, May, 1907, is an article by A. S. D. Maunder, entitled "An Apparent Influence of the Earth on the Numbers and Areas of Sun Spots in the Cycle 1889–1901." The article is made up from photo-heliographic results published by the Astronomer Royal in the Greenwich Observations. These results consist of the measurements of sun spots on photographs of the sun taken daily at Greenwich, at the Kodaikanal Observatory, in India, and at Mauritius. The question considered by Mrs. Maunder is where, and especially on which side of the sun, do most of the sun spots originate? The wholly unexpected result was that more spots come into view around the east limb of the sun than pass out of view around the west limb. That is, more spots die out on the visible side of the sun than are formed on it.

As a summary of Mrs. Maunder's work we have the following table:

Spots born on visible hemisphere,	394.
" " " invisible "	572.
Spots died on visible hemisphere,	564.
" " " invisible "	402.

All told, 947 groups came into view around the east limb or formed close to it, while only 777 groups disappeared around the west limb or dissolved close to it. This leaves a difference of 170 or 22 per cent. of the disappearances which must be laid to some influence exerted by the earth. Mrs. Maunder says:

> These disproportions either in area or in number can not be put down to any cause connected with the history or growth of the spots themselves, or to any solar cause whatever. East is east and west is west solely from an earthly point of view. From the solar standpoint, the east limb, central meridian and west limb are purely conventional lines behind which every portion of the sun's surface moves; they are not fixed landmarks upon the sun. The cause of the disproportion must be terrestrial and terrestrial only.

In *Observatory*, London, **42**, 51–2 (1919), is an article by Evershed, then director of the Kodaikanal Observatory, on "The Displacement of Solar Lines Reflected by Venus," in which the author finds that

[4] *Ann. d. Phys.*, **30**, 57–136 (1909).

[5] *Zeitsch. f. Phys.*, **28**, 299 (1924); **33**, 273 (1925); **34**, 433 (1925); **35**, 757 (1926) and **37**, 342 (1926).

[6] *Astrophys. Jour.*, **64**, 136 (Sept., 1926).

the mean wave-length of the iron lines in the light reflected from Venus is

quite appreciably smaller when the angle Venus-Sun-Earth exceeds 90 degrees. At 45 degrees the sun-arc displacements are nearly the same as in ordinary sunlight, but there is a progressive diminution of wave-length as Venus passes round toward superior conjunction; and those lines which ordinarily show larger displacements toward the red than the average diminish in wave-length more than those lines which show smaller displacements. The September series, taken from a hemisphere of the sun turned 135 degrees from the Earth, show a shift to the *violet* of the solar Fe lines.

. . . .

Whether we like it or not it seems necessary to admit that the Earth does affect the Sun, causing a movement of gases analogous to that taking place in a comet. Is it possible that this action controls to some extent the distribution of sun spots and prominences, which seem also to betray an earth influence?

It has long been suspected that the occurrence of sun spots is influenced in some way by planetary configurations, though the small gravitational influence of the planets upon the sun seemed to make such an influence improbable. In *Philosophical Transactions* of 1869 and 1870 and in *Philosophical Magazine* of 1870, De La Rue, Stewart and Loewy give data for every day for a long term of years on the portion of the visible hemisphere of the sun which was covered with spots. These records are from solar photographs which were made at Kew and were all carefully measured. In addition to their own records they used the most reliable records back to 1832. They give the variation in millionths of the sun's visible surface from the mean spottedness for the period of the investigation, the plus sign indicating an excess and the minus sign a deficiency of spottedness as compared with the mean.

They then relate these data to planetary configurations, and give a table showing the relative spottedness of the sun for each 30 degrees of the orbit of Mercury, starting at perihelion, and for each 30 degrees of separation of Jupiter and Venus, Jupiter and Mercury, Venus and Mercury and Jupiter and Mars. The earth was not included in this comparison. In the following table their data are tabulated by combining the two sets taken when the planets are at the same angular distance on opposite sides of their conjunction, and in the case of Mercury, when the planet is at the same angular distance from perihelion on opposite sides of its orbit.

TABLE I

PLANETARY CONFIGURATIONS AND SUN SPOTTEDNESS

Angular separation	Jupiter and Venus	Venus and Mercury	Mercury and perihelion	Mars and Jupiter
0– 30	+1107	+1979	+103	−136
30– 60	+ 210	+ 89	+ 47	− 76
60– 90	− 191	− 718	+ 74	−220
90–120	− 421	− 962	− 90	−369
120–150	− 728	− 885	−389	−348
150–180	− 826	−1042	−773	−453

The data given in Table I seem to show conclusively two facts concerning the influence of planetary configuration upon sun spottedness. They show, in the first place, that the spottedness of the sun *is* affected by the relative positions of some of the planets, and, in the second place, that the planetary effects are not due to gravitational tides; for the tide-raising influence of two planets would be combined both at conjunction and at opposition, while their resultant influence would be at a minimum when their angular separation was 90 degrees. On the other hand, if the planetary influence is of the nature of electrostatic induction the resultant effect of two planets will be a maximum when they are in conjunction and a minimum when they are in opposition, as is seen to be the case.

The comparison of Jupiter-Venus with Mercury-Venus indicates that Mercury is more effective than Jupiter in the production of sun spots. Since the electric potentials of the different planets are not known, it is impossible to compute their relative inductional effects upon the sun. If we assume them to be charged to the same potential, their separating effect upon the sun's charge will be as follows, calling that of the earth 1: Mercury, 6:5; Venus, 2.6; Earth, 1; Mars, 0.16; Jupiter, 0.08. This leads us to suspect that the three inner planets are the ones most influential in the formation of sun spots.

The relation of planetary configurations to the periodicity of sun spots deserves more consideration than can be given it in this paper.

Since it is now possible to detect cyclonic disturbances upon the sun which are not sufficiently intense to produce visible spots, perhaps a test of the theory here set forth might be made by observing whether the region of the sun opposite to the planet Mercury is especially susceptible to cyclonic disturbances.

FERNANDO SANFORD

PALO ALTO, CAL.

EXPERIMENTS WITH BACTERIAL FILTERS AND FILTERABLE VIRUSES

WE have come to divide bacteria and viruses into filterable and non-filterable, and we have come to think that those organisms, visible or invisible, which are smaller than the pores of our filter are filterable. That size, however, can not be the sole criterion we

have known from the behavior of certain aniline dyes. Thus Victoria blue, a basic dye, will not pass a Berkefeld filter while Congo red, an acid dye, will readily pass through the same filter. Now it happens that the filters which we use in bacteriologic practice, namely, sand, porcelain and diatomaceous earth, are all of them some form or compound of silicic acid, so that really when one speaks of a filterable organism, dye or other colloid, one ought to say filterable through siliceous filters.

One may speak of a filter when it is in action as a suspension of the material of which the filter is composed, in the fluid which is being filtered. Now silica has a definite negative charge and it may be that if one constructs a filter of a material of charge opposite to that of silica, one might find that bacteria or colloids which are filterable through silica filters are non-filterable through such other filters, and *vice versa.*

Accordingly, filters were made of plaster of Paris and experiments with various dyes and viruses were made. Thus Victoria blue which does not pass a Berkefeld or siliceous filter readily passes through a filter made of plaster of Paris; while Congo red, which readily passes through the Berkefeld filter, does not pass through a plaster of Paris filter. If, however, we make a dilute solution of Congo red and render it very slightly acid, thereby changing the color to blue, we find that the blue dye does not pass through the Berkefeld filter but does pass through the plaster of Paris filter. In other words, by reversing the electrical charge one has reversed the filterability.

Acid and basic dyes should be used to test filters. In this work, any Berkefeld filter which does not completely remove Victoria blue from the solution is rejected. The gypsum filter is tested with Congo red.

Now when we came to consider the nature of plaster of Paris a very interesting phenomenon was found. Plaster of Paris is supposed to be calcium sulfate, but when filters were made of calcined chemically pure calcium sulfate, it was found that such filters had no action on any of the colloid dyes used. Both Victoria blue and Congo red readily passed through such filters. Calcium sulfate is neutral and without charge. Then it was found that the plaster of Paris of commerce contains up to 5 per cent. of calcium carbonate, and when calcium carbonate was added to our chemically pure calcium sulphate and filters made from this mixture, such filters acted as did the filters made of commercial plaster of Paris. The calcium carbonate is alkaline and has a positive electrical charge. It is probable that the calcium sulphate in our filters acts as a binder for the calcium carbonate and that it is the calcium carbonate which is the active adsorbing component of our filter.

It was thought that, if a filter was made of plaster of Paris and silica or diatomaceous earth intimately mixed before setting, we might produce a filter which would remove both positively and negatively charged dyes and colloids. When this was done, however, it was found that such filters were neutral and that neither category was filtered out. Evidently the calcium carbonate and silica neutralized each other. If, however, the filter was made so that the core was a Berkefeld filter with a cortex of plaster of Paris poured over this, so that the contact of materials was only at the joint surface, then it was found that dyes, viruses and colloids of both positive and negative charge could be removed by filtration through this apparatus. The outer layer when made of plaster of Paris removed the acid or negative particles, and the inner or siliceous core removed the basic or positive particles.

It was found that one could increase the efficiency of the basic or gypsum filter by incorporating in the plaster of Paris up to 25 per cent. of magnesium oxide, calcined at 1300° C.

Experiments were made with so-called filterable microorganisms and viruses which were available and the following all failed to pass the gypsum filter: The bacteriophage of *Staphylococcus aureus,* the *Vibric percolans* of Stuart Mudd, mosaic disease of tobacco, vaccine virus and the virus of rabies.

It was also found that one could remove various bacterial toxins by filtration. The following were removed by filtration through the gypsum filter with 25 per cent. magnesium oxide: Diphtheria toxin, botulinus toxin, abrin and tuberculin. Tetanus toxin is unaffected by filtration through any gypsum filter, but, as has long been known, is removed in limited quantities by passing through siliceous filters.

Experiments were also made in filtering with both gypsum and siliceous filters watery infusions of various neurotoxic drugs with the idea of removing, if possible, their neurotoxic components. A large number of such experiments were made, but the details will be given in another communication. However, I wish to say here that both with bacterial toxins and toxic components of various neurotoxic drugs the following rule has held: namely, that all narcotizing toxins and drugs are filtered out with the basic or positively charged filter and the spasm-producing toxins and drugs come out with the siliceous or acid or negatively charged filter. And this makes it strongly probable that the respective nerve cells involved in the production of the above two classes of symptoms have a similar though opposite difference in reaction or charge.

S. P. KRAMER